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EDITORIAL

The Journal of Nervous and Mental Disease was launched in 1874 under the guidance of Dr. J. S. Jewell, "Professor of Nervous and Mental Diseases" in the then Chicago Medical College. The introductory editorial of Volume 1, Number 1, called attention to the fact that "nervous and mental diseases are becoming far more prevalent then formerly. This is not due simply to an increase of population; the causes of the increased prevalence of such diseases lie far deeper, and deserve most careful study." This somewhat debatable statement set the tone of the editorial policy.

JNMD appeared quarterly in its first few years. With self-conscious pride it printed lists of exchanges with foreign journals, virtually all of which are now defunct. At the beginning it leaned heavily on "translations of articles of merit" and selections from foreign periodicals. A book review section soon appeared; roughly half the appraisals were of foreign publications. In criticizing one new book the anonymous reviewer, presumably the editor, expressed his feelings bluntly as follows: "To justify the appearance of a new work...it should be able to lay fair claim to either something new in matter or in method. But so far as this work is concerned, it can lay claim to neither. It is published because the author desired it should be, and because he could find a publisher." The reviewer today aims to be equally forthright but less personal in his criticisms.

After a succession of editorships of relatively short tenure, Dr. Smith Ely Jelliffe became both editor and owner. Ever since then JNMD's goal has been to integrate the variables that operate on the level of psychological experience with those which result from structural and biochemical changes both in the nervous system and in the body in general. This depends upon the cooperation of neurophysiologists, neurobiochemists, and analytically sophisticated behavioral scientists. Cooperation between such diverse groups requires either a hypothetical freedom from bias or else an honest and good-humored insight into the distorting role of those biases to which all of us are vulnerable. Furthermore the experimentalist in these subtle fields must constantly be on guard to correct a built-in tendency to oversimplify the complexities of human behavior in his effort to fit them into the simpler framework of the laboratory. Similarly the behavioral scientist must be alert to the ever-present danger of treating refined description as though it was explanatory, adjectives as though they were nouns, abstractions as though they were power-packs. Thus The Journal of Nervous and Mental Disease will attain its goal only through the conjoint efforts of behavioral scientists who are free from the illusion of omnipotence of mind and who therefore do not suffer from a derived organo-phobia, who will therefore be able to work together with organicists who are free from the illusion of the impotence of mind and who therefore do not suffer from a derived psycho-phobia.

In the meantime, as we struggle towards this ideal, the makeshift marriage between psyche and soma will continue to have its ups and downs, through a succession of tentative unions followed by temporary separations, during which gradually and

hopefully each discipline will become mature enough for a fruitful and lasting union. This can be achieved only by hard work, humility, honest self-criticism and that eternal vigilance which freedom always demands, whether it be freedom from bias or freedom from more subtle tyrannies. This will not be achieved by banners and slogans.

As we think back over this long road, it may be of historical interest to note that long before there was a "*Life*" a la Luce, there was a more or less humorous weekly of the same name, reminiscent of the British "Punch." This ancient and sometimes humorous "*Life*" from time to time dealt with serious topics as well. Indeed in 1891 we find it dealing in a rather caustic manner with the reluctance of doctors in general to face up to the mind-body problem. Thus in September of that year, on the editorial page of the 455th number, 18th volume, appear the following words:

"There is an increasing number of stories of hospital cases in which physical results are sought by means of impressions made upon the mind. There seem to be a good many cases in which if the mind can be cured, a cure of the body will follow. But the cure of the mind is a matter fit for the most expert handling, and is a profession in itself.

"The truth is that it seems to be becoming apparent that the body and the soul are too inextricably mixed up ever to be successfully dealt with apart from the other. The man who cannot tackle both and make them work together for their proprietor's comfort may as well resign himself to see a certain class of patients, or at least a certain class of diseases, go up the front steps of his competitor who can."

The Journal of Nervous and Mental Disease is hardly worried about whose front steps will be ascended by the larger number of patients. It is deeply concerned, however, with the open mind and the open heart, which can make of each investigator and of each clinician a bridge between two fields which have too long been viewed as irreconcilable. It is to this goal that this journal will continue to dedicate its pages.

Lawrence S. Kubie, M.D.

EVIDENCE FOR FUNCTIONAL GENESIS IN MENTAL ILLNESS: SCHIZOPHRENIA, DEPRESSIVE PSYCHOSES AND PSYCHONEUROSES[1]

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The etiology of mental illness is complex. The present investigation provides a demonstration of a functional or psychogenic factor in the precipitation of certain forms of severe disturbance. This does not deny the plausibility of hereditary or biochemical factors that may make one person more vulnerable than another, but it does show how mental illness may be precipitated in one of two equally vulnerable persons if the strains of living prove more severe for that person. In presenting a demonstration of a psychogenic factor (the death of a parent in childhood) with a statistical test of a specific hypothesis, this investigation calls attention to the necessity of continuing a search for psychogenic factors, no matter what supporting evidence may accumulate for hereditary or biochemical concomitants in mental illness.

We are familiar with many types of anniversaries in everyday life. In this paper, however, the type of anniversary we are proposing to explore, namely, long-delayed effects of parental death dating from the patient's childhood, is usually obscure in its manifestations and rarely understood. While we have found events in childhood other than parental death—such as parental psychoses, marked home disturbances during oedipal and adolescent years, or sibling deaths—to have anniversary significance in relation to the next generation of children, we shall be concerned here only with tracing some aftermaths of the death of a parent sustained before the age of 16 years.

Because an age in one generation serves to reactivate a trauma that occurred at the same age in an earlier generation, we have termed these age coincidences "anniversaries" and have designated our hypothesis an anniversary reaction precipitated by children (5).

In testing a limited hypothesis, of necessity we must ignore many facets. We shall be studying only the psychoses and hospitalized psychoneuroses, not other forms of symptomatology such as psychosomatic illnesses or the less severe psychoneuroses. We are concentrating on only one type of anniversary precipitated by children, that connected with child-age coincidences, not other forms such as parent-age coincidences which also occur in our data. Finally, it is clear that reactions to a parental death suffered in childhood present a broad spectrum of responses, only one of which is dealt with in this paper, the much-delayed anniversary. Closely related research by Fleming and her collaborators (2, 3) bears on the importance of the reactivation of mourning in the course of the psychoanalysis of adults who sustained the loss of a parent in early life.

THE ANNIVERSARY HYPOTHESIS

The following hypothesis is here subjected to test: If a person has lost a parent by death in childhood, and that person subse-

[1] This research was supported from 1954 to 1959 by grants from the National Institute of Mental Health. It has continued as one of the programs in the Laboratory of Human Development at Stanford University. We wish to express our appreciation to Dr. Ernest R. Hilgard, Director of the Laboratory, for general guidance in statistical planning. To Dr. Lincoln E. Moses of the Department of Statistics and to Dr. Quinn McNemar of the Department of Psychology, Stanford University, we are grateful for timely assistance in the treatment of data.

Excellent cooperation afforded us by the staff at Agnews State Hospital made this study possible. We wish to mention in particular Dr. Walter Rappaport, the Medical Superintendent, Dr. Robert Quirmbach, the Clinical Director, and Helen Hargrove, Director of Social Service.

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quently marries, has children, and later is hospitalized for mental illness, the first hospitalization is likely to occur beyond chance expectancy when the oldest child of that person is within a year of the age the person was when the parent died.

Our hypothesis is so stated that it can be tested objectively with public data (ages, death dates, admission dates) and thus avoids the pitfalls of inference, which is implicit in the clinical approach. If it can be shown that the relationships occur on a nonchance basis, then we will have firm evidence bearing upon the psychogenesis of mental illness in these special circumstances. Despite the many conjectures making psychogenic influences plausible, very few of our psychiatric data permit objective demonstration. Thus positive statistical findings would give weight not only to the anniversary hypothesis, but, we submit, to the general problem of functional causation in mental illness as well.

The hypothesis arose both out of clinical observations and some published case studies showing the possible dynamic relations involved (6, 7). The statistical analysis, however, does not involve this case material.

The following case is presented to illuminate the kinds of relationships that are explored in the statistical study.

THE CASE OF JUNE WILSON

June Wilson lost her mother by death when she was nine years old. She herself entered the mental hospital for the first time, depressed and following a suicide attempt, when her eldest daughter, Janie, was eight. Because these two ages—June's childhood age of nine at the loss of her mother, and Janie's age of eight when June was hospitalized—are within a year of each other, the case qualifies as an "age-coincidence" as we have defined it. Without any supplementary clinical material, the relationship appears either to be one of chance or to have a kind of 'magical' quality. The clinical material, however, shows the coincidence to be understandable.

June's childhood experiences made her mother unusually important to her, so that the loss became a very traumatic one. The mother had suffered a "stroke" at the time of June's birth and was incapacitated for two years. Cared for by various relatives, June lacked normal mothering during the first two years of her life. After her return she clung to her mother as an important support for the next few years, until her mother died. Interwoven in June's feeling both before and after the death was a motif of guilt: she felt she had caused her mother's illness. Her mother had had six children before her without incident; with June's birth the mother had the "stroke" and suffered from hypertension for the next years until, approximately a year before death, she became depressed, complained of headaches and dizziness, and died. June was nine at the time.

We find that in some cases of parental death there are healing circumstances afterward, but these were not found in June's case. The home disintegrated; the father took no responsibility for the children. June went away to live with an older unmarried sister for a while, then with an aunt's family, and finally in a brother's home. She received little mothering in these homes; her mother was never really replaced.

It is characteristic of these cases that little pathology is noted in the personality of the growing child. Other than that she appeared somewhat withdrawn, June is reported to have managed all right. She took nurse's training, began to practice as a nurse, married, continued as a nurse for ten more years, and was a good mother to her two daughters, Janie and Mildred, who were three years apart. While the marriage was a chronically unhappy one, it endured without any special crises.

Trouble began to appear when Janie approached the age June was when her mother

died. The relatives report that Janie reminded them of June when she was that age, and no doubt Janie also reminded June of herself.

As June became disturbed and depressed, she began to have many of the symptoms her own mother had had the year before her death. June complained of headaches and dizzy spells, like her mother's, and she reported a feeling of impending death, as though she might suddenly "drop over." Her mother had "dropped over" with a stroke, although the cause of death was attributed to uremia. Uremia with kidney involvement frequently includes back pains; June also complained of back pains. She kept saying: "I think I'm going to die." Physical examinations were negative.

Thus we see June fearing that she is to go as her mother went, now that her daughter has grown to the age she was when her mother died. Her attempted suicide and her psychotic break are perhaps symbolic fulfillments of the death that she feared as inevitable.

To be sure, there are many cases of parent loss not followed by a disturbance such as June's. Some of the following factors appear to make cases like June's occur with a frequency that can be detected statistically: insecurity in the home prior to the loss, a close ambivalent tie to the lost parent with unresolved guilt, and a failure of adequate replacement. We need not comment upon these in detail. We recall that during June's first two years of life, the important function of mothering belonged to different relatives, some more interested than others. June's tie to her mother during the next six to seven years was the one strong continuous support of her life, so that the mother's death marked the end of an era for this nine-year-old child. There was no subsequent mothering of any substantial nature. Among sources of guilt we note that the mother's illness began with June's birth and that the child carried a deep feeling of responsibility. With these considerations, it becomes clear how such a death, both consciously and unconsciously, can become a landmark.

The background factors in this case history were not conducive to growth toward emotional maturity. Emotional maturity can be distinct from continued advancement in intellectual and professional areas. June remained competent professionally at the same time that she was relatively withdrawn in her relations with people. Although she had married and had children, in many ways she had remained a child, tending to identify strongly with her own children, and particularly with the one who resembled her. When that child reached the age she was at the death of her own mother, she began to relive this part of her past life both in her role as mother, identified with her own mother, and as a child, identified with Janie.

The anniversary hypothesis as stated, and as illustrated by the June Wilson case, calls for some additional comments. There lie behind it certain conceptions of the influence upon a child of such a traumatic event as the death of a parent, and the role of a child in the next generation as a "reminder" of this trauma. In order to achieve objectivity for the purpose of statistical treatment, some arbitrary decisions had to be made in formulating the precise hypothesis to be tested. For one thing, we have allowed a leeway of a year on either side of the target age, recognizing 1) that age at hospital admission is not a precise indicator of the time of onset of symptoms, and 2) that a child of one generation may serve as a reminder of the child of another generation as a consequence of developmental level not accurately reflected by chronological age. Furthermore, we have chosen the eldest child for purposes of statistical comparison because this permits the unambiguous inclusion of the one-child family. Clinical data can often give us evidence of re-living phenomena that help us to distinguish between chance and non-chance coincidences, but such evidence is not used

in making the statistical tests. We know, for example, that there are statistically positive cases where age coincidences occur, in which there is no clinical evidence supporting the hypothesis; at the same time, there are reliving experiences associated with other children in the family (*e.g.*, a like-sexed child later in the sibling order) that give clinical support to the hypothesis, although statistically such a case becomes a negative one because the hypothesis is being tested with the eldest child as the target. By keeping the hypothesis relatively simple for statistical purposes some richness is lost; but if the hypothesis is supported statistically, supplementary clinical inferences can be made with greater confidence. The statistical operation can indicate whether anniversaries occur in numbers not attributable to chance: if that can be established, we need detailed study of individual cases in order to use the findings for the understanding and treatment of these cases.

Before turning to the statistical analysis, we may point to one further illustration of kinds of evidence that cannot be applied to statistical analysis. In the following case the anniversary hypothesis is supported not only with the first child but with the second as well.

Martha Newell was an only child whose mother died when she was 13 years old. When Martha's eldest child, Mark, became 13, she was admitted to the mental hospital for the first time, and released after a few months. Three years later, however, when her daughter Mary became 13, she was again admitted to the hospital. We noted from the case records and her own comments that she had deliberately given both children names similar to her own, and both children were said to resemble her in personality.

To the extent that the case material bears out the importance of the anniversary events, and provides supplementary detailed information, we gain knowledge of the causal factors, their role in the precipitation of illness, and suggestions for therapy. This paper, however, is but one of several reports in process, and is concerned with the statistical data bearing on the anniversary hypothesis.

SELECTION CRITERIA

Cases to be studied for the occurrence of age-coincidences were found by screening all admissions to Agnews State Hospital for nine-month periods during each of four years in 1954, '55, '56 and '57. Patients entering during these periods totalled 8,680. The hospital social service anamnesis of each case was reviewed in order to determine which cases should be included; in a relatively small percentage of cases the patient's stay in the hospital was so brief that an anamnesis was missing or there were no relatives available for interview. In order to avoid any bias, every case meeting predetermined criteria was included. These criteria were as follows.

1. The patient must be native-born, white, diagnosed as psychotic or neurotic. It may be noted that patients with only very severe neuroses are hospitalized. Diagnoses excluded were: alcoholic, psychopathic, organic. Alcoholism, a frequent diagnosis, was omitted because it was thought that alcoholism over the years would obscure any sudden impact of an anniversary reaction. The psychopathic and organic cases also seemed inappropriate to be used in testing our hypothesis.

2. The patient must have been married, a parent, and under age 50 at this admission.

3. While this admission need not have been the patient's first, the first admission must have occurred after becoming a parent. Only the date of first admission was used in the statistical age-coincidence study. Where the first entry was elsewhere, careful studies of the other hospital records were made and supporting information obtained from relatives.

4. The patient must have lost a parent by death between the ages of 2 and 15, inclusive.[3] The date of loss was investigated as

[3] At first the selection was made through age 18. The reasons for the change are stated later.

thoroughly as possible by interviews with the patient and relatives or by letters to relatives.

The resultant sample, obtained on the bases of these criteria, derives from all current cases for which the relevant data were available.[4]

NATURE OF THE SAMPLE

The total pool of 8,680 patients, those admitted to the hospital for four years during the months that cases were reviewed, was screened. This pool was reduced to 2,402 by the requirement that patients must be native-born, white, under 50, and diagnosed as psychotic or neurotic. This group (941 males and 1461 females) we considered the special universe of patients from which our experimental sample was drawn, and demographic characteristics of our experimental sample will be compared with it.

The experimental sample, or parent-loss group, was arrived at by the following steps. The total group of 2,402 was reduced by more than half by the requirement of marriage and parenthood. Of the 941 males only 260 (28 per cent) were married and had children at first admission, while of the 1,461 females, 757 (52 per cent) were married and had children. This sex difference in marriage and parenthood among male and female psychotic and neurotic patients is of interest, but is not relevant to our study except to the extent that it reduces the size of the male sample much below that of the female one.[5]

This residual sample of 260 males and 757 females was further reduced by the requirement of parent loss in childhood. Those cases meeting our final criteria of parent loss between the ages of 2 and 15 inclusive, left 40 male patients (15 per cent of the 260), and 153 women patients (20 per cent of the 757). Thus the experimental sample, in number 193, represents eight per cent of the psychotic-neurotic population of 2,402.

The final experimental sample was found not to differ significantly from the parent population with respect to education, occupation, religion, and proportion of patients in the various diagnostic categories.

Diagnostic categories: The diagnoses in the experimental sample are compared with the total hospital admission cases in these categories in Table 1. The requirement of marriage has decreased considerably the proportion of schizophrenic men in the sample, with a smaller decrease among the women. The sample is roughly equivalent to the general hospital admission population; three-fifths of the sample are diagnosed as psychotic, one-fifth as psycho-neurotic. These are the diagnoses entered upon the official hospital records and suffer the limitations of any such recorded diagnoses.

Education and occupation: Cases admitted to the State Hospital seemingly no longer differ much in educational and occupational status from the population at large. The median educational level (Table 2) is high school graduation, which is above the general level of the white urban population 25 years and over as reported in the U. S. Census for 1950. Similarly, the 21 per cent with some college education is above the 13 per cent reported for the population as a whole. Because the level in the urban West (where the State Hospital is located) is somewhat higher than the U. S. as a whole, it is a reasonable conjecture that the hospital population now represents a fair cross-section of educational

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[4] Examination of the large number of case records has provided material for the study of other features of mental illness, such as alcoholism, the extent of parental death in various types of mental illness, the effect of parental loss through hospitalization for psychosis, but these other uses of the data are not treated here.

[5] An additional 930 patients, native-born, white, and under 50 were diagnosed as alcoholic. Of these, 679 were male. In contrast to the psychotic males where only 28 per cent were married with children, 61 per cent of the males with alcoholic diagnosis were parents before admission to a mental hospital. This contrast was discussed in a paper entitled "Some Psychodynamic Equivalents Between Alcoholism in Men and Schizophrenia in Women," read before the American Psychiatric Association in San Francisco, May, 1958.

TABLE 1

Diagnoses of Sample Cases Compared with Total Hospital Admission Cases in Psychotic-Neurotic Categories

Diagnosis	Male		Female		Both	
	Total	Experimental Sample	Total	Experimental Sample	Total	Experimental Sample
	%	%	%	%	%	%
Schizophrenic	77	52	69	62	72	60
Psychoneurotic	12	23	18	18	16	19
Manic-Depressive; Psychotic Depressive	10	20	8	13	9	14
Involutional	1	5	5	7	3	7
	100 N = 941	100 N = 40	100 N = 1461	100 N = 153	100 N = 2402	100 N = 193

TABLE 2

Educational Level of Sample Cases Compared with Total Hospital Admission Cases in Psychotic-Neurotic Categories

Educational Level	Male		Female		Both	
	Total	Sample	Total	Sample	Total	Sample
	%	%	%	%	%	%
Less than 8th Grade	8	7	7	5	7	6
8th Grade	13	30	9	14	11	17
Some high school	27	25	26	31	27	29
High school grad.	26	20	36	34	32	31
Some college	13	10	12	10	13	10
College grad.	7	3	6	4	6	4
Post-grad.	3	5	2	2	2	3
Not ascertained	3	—	2	—	2	—
	100 N = 941	100 N = 40	100 N = 1461	100 N = 153	100 N = 2402	100 N = 193

levels.[6] The experimental sample is roughly similar to the more general admission population.

The occupational distribution (Table 3) confirms the educational one in showing the generally representative level of the hospital population. The higher occupational groups (professional and managerial) provide 15 per cent of the hospital admission total, while the U. S. Census for 1950 lists 22 per cent in this group for the urban population. The semi-skilled, unskilled, and unemployed constitute 35 per cent of the hospital admission group, while the Census lists 38 per cent in these categories. The experimental sample is again a cross-section of the hospital total, the chief discrepancy being the shift between clerical-sales and skilled for the women when the total group and sample are compared. This shift is a sensible one in view of the exclusion of unmarried women from the smaller sample, and the classification of their occupational level by that of their husbands. Put another way, single women who are in clerical-sales occupations tend to marry men more often classified as skilled workers. The sum of clerical-sales and skilled is essentially

[6] A sample survey in a metropolitan area that supplies most of the patients to this hospital has been conducted and confirms the assertion that the hospital patients match closely the educational level of the community in the same age brackets.

TABLE 3

Occupational Level of Sample Cases Compared with Total Hospital Admission Cases in Psychotic-Neurotic Categories

Occupation (Husband's Occupation for Married Women)	Male		Female		Both	
	Total	Sample	Total	Sample	Total	Sample
	%	%	%	%	%	%
Professional	7	8	8	10	7	9
Managerial	8	12	8	7	8	8
Clerical-Sales	14	12	24	12	20	12
Skilled	21	30	17	28	19	29
Semi-skilled	13	18	11	14	12	15
Unskilled	30	20	15	13	21	14
Unemployed	2	—	2	2	2	2
Not ascertained	5	—	15	14	11	11
	100 N = 941	100 N = 40	100 N = 1461	100 N = 153	100 N = 2402	100 N = 193

TABLE 4

Religious Preference of Sample Cases Compared with Total Hospital Cases under Age 50

Religious Preference	Male		Female		Both	
	Total	Sample	Total	Sample	Total	Sample
	%	%	%	%	%	%
Protestant	49	62	50	49	50	52
Catholic	34	35	37	36	36	35
Jewish	1	—	2	1	1	1
Other	7	3	7	12	7	10
None and not ascertained	9	—	4	2	6	2
	100 N = 2027	100 N = 40	100 N = 2019	100 N = 153	100 N = 4118	100 N = 193

alike in the total hospital admission group of women and in the sample of women.

These demographic characteristics have been given in some detail to avoid the misinterpretation that a State Hospital population primarily represents lower educational and income levels. (It should be recalled that under consideration here is the current admission population, not custodial care patients.)

Religion: Because the study is concerned with parent-child relationships and attitudes toward death, it is conceivable that the practices among different religious groups would be important factors. Therefore our sample has been compared in religious preference with that of the total hospital group (Table 4). The total in this case includes all admission cases under age 50, and is not limited to the diagnostic categories represented in the sample. Because these data were available, and because the sample corresponds so closely to the general distribution, it was felt unnecessary to re-examine the data by diagnostic category. The group is primarily Protestant and Catholic, with very few Jewish patients. There was, in fact, only one Jewish woman in the experimental sample, and no Jewish men. The "other" groups are primarily Christian sects, such as Christian Science, Jehovah's Witnesses and Pentecostal groups.

The fact that the active sample is representative of the hospital as a whole, and the

TABLE 5

Age at Death of Parent of Psychotic and Psychoneurotic Patients Married and with Children at Time of First Admission to a Mental Hospital

Patient's Age at Death of First Parent	Male (Losses among 260 patients)				Female (Losses among 757 patients)				Both (Losses among 1017 patients)
	Father Loss	Mother Loss	Both Lost	Total	Father Loss	Mother Loss	Both Lost	Total	Total Losing One or Both Parents
Under 2 years	1	1	—	2	8	10	1	19	21
2–15* years	18	17	5	40	82	65	6	153	193
16–18 years	1	4	—	5	18	14	2	34	39
Information incomplete	3	3	—	6	2	3	2	7	13
Total	23	25	5	53	110	92	11	213	266

* This group selected as the sample for study.

hospital is representative of the community, gives cogency to whatever generalizations may arise from the findings.

PARENT LOSS

To facilitate objective definition of "loss of a parent in childhood," all cases in the prospective sample losing a parent through age 18 were tabulated (Table 5).

Fifty-three of 260 men, or 20.4 per cent, had lost one or more parents between birth and age 19; 213 women among 757, or 28.1 per cent had suffered similar loss. While such figures have been used in the past to test the hypothesis that parent loss in its broad context may help precipitate mental illness, *e.g.* recent summary of studies by Gregory (4), the absence of good control data has made the results difficult to interpret.

We made a control study of a metropolitan, non-hospital sample in order to obtain comparison figures. This non-hospital sample showed that 21.1 per cent had lost a parent by death prior to age 19 (8). The hospital sample of psychotic-psychoneurotic patients under age 50 yielded a rate of 24.2 per cent. Overall, then, the hospital sample is not very different from the community at large in respect of parent loss. The indirect computations from Census data, as made by Shudde and Epstein in 1954 (11) appear to underestimate markedly the amount of orphanhood. According to their computations, children under 18 in 1930 (which would correspond closely to the age bracket of our sample) had lost parents in 11.7 per cent of the cases, and, according to their figures, the rate was decreasing markedly, so that a smaller per cent should be found among our younger patients. Our data, both for the hospital and for the community outside, show a rate almost double theirs.

While we originally obtained figures for the larger group, the final decision was to use the age of loss between two and 15 years, inclusive, thus reducing the number of patients with parent-loss as so defined. The reasons for this limitation are as follows.

1) The presence of post-partum disturbances within the first few months after birth might be misinterpreted as an anniversary for a patient who had lost her mother in the first year of her life. Hence we felt it best not to consider parent losses in the first year. Also the psychological impact on the infant of loss in the first year, prior to the development of language, seemed to us to involve factors discontinuous with those of later losses, in which some open memory of the loss could be manifested. For both these reasons, parent losses under the age of two were excluded.

2) We found that between the ages of 16 to 19 many of the patients had begun to live lives of their own, away from the parental home: some away at school, some at work, some already married. It seemed wise, therefore, in testing the age-coincidence

hypothesis, to deal with the age-range in which loss of a parent would be clearly disruptive of home nurturance. We therefore set the sixteenth birthday as the upper limit, including in the sample only those who had lost a parent prior to this date.

The few cases listed in Table 5 as "information incomplete" are so classified when the available data indicated that the case belonged, but the information was not precise. For example, the record might say "mother died when she was a little girl," or "patient had a child by a previous marriage, age unknown." If it was not possible upon inquiry to establish the relevant facts, the case was listed as incomplete and was of course excluded from the final experimental sample.

THE ANNIVERSARY HYPOTHESIS

The hypothesis states that the presence of a child of the age the patient was when parent-loss occurred might have an effect in precipitating mental illness at that particular period of life. It is necessary, then, to determine in how many of these cases the patient entered a mental hospital for the first time when the oldest child was within a year of the patient's age when the parent died. As indicated above, we have chosen the age of the oldest child alone to make the statistical analysis more straightforward, although the clinical data show that it is not always the oldest child that is the significant one in these terms.

The parental deaths considered in the study are given in Table 6, presented according to the age of the patient at the time of death, the sex of the patient, and the sex of the parent who died. The numbers of age coincidences to be used in testing the anniversary hypothesis are also given.

The age-coincidences listed in Table 6 are given according to the diagnostic categories shown in Table 7. The 28 coincidences within a group of 193 cases represent 14.5 per cent of the cases, or about one in seven. Of these, 21 of 28 derive from patients diagnosed as psychotic, seven of 28 from patients diagnosed as psychoneurotic. There are, of course, fewer psychoneurotic cases in the sample, so that between psychotic and psychoneurotic diagnoses the prevalence of coincidences is relative to the ratio of the sample.

Some estimate of chance expectancy is

TABLE 6

Age at Loss of Parent and Frequency of Age-Coincidences

Patient's Age at Loss of Parent	Female				Male			
	Lost Mother		Lost Father		Lost Mother		Lost Father	
	Cases	Age-coincidences	Cases	Age-coincidences	Cases	Age-coincidences	Cases	Age-coincidences
2	7	2	7	1	0	0	3	0
3	4	0	6	1	1	0	1	1
4	2	0	2	0	1	0	1	0
5	2	0	8	2	1	0	1	0
6	7	0	3	0	1	0	2	1
7	4	1	9	0	3	0	1	1
8	10	4	9	2	2	0	1	0
9	5	2	6	0	2	0	2	0
10	6	1	2	0	2	0	1	0
11	1	0	4	1	3	0	1	1
12	4	0	4	1	1	0	0	0
13	3	1	8	0	0	0	1	0
14	4	1	5	0	0	0	2	0
15	6	2	9	1	0	0	1	0
Total	65	14	82	9	17	0	18	4

TABLE 7

Coincidences Between Patient's Age at Parent Loss and Age of Oldest Child at First Admission to a Mental Hospital, by Diagnosis

Diagnosis	Men			Women			Total Coincidences
	Father Loss	Mother Loss	Both	Father Loss	Mother Loss	Both	
Schizophrenic	3	0	0	7	8	0	18
Psychoneurotic	0	0	0	2	4	1	7
Manic-Depressive; Psychotic Depressive	1	0	0	0	2	0	3
Involutional	0	0	0	0	0	0	0
Total coincidences	4	0	0	9	14	1	28
Size of Sample	18	17	5	82	65	6	193

necessary to determine the significance of the 14.5 per cent of coincidences found. The only samples numerically large enough to justify a statistical test are those of the women who lost fathers in childhood (N = 82) and the women who lost mothers in childhood (N = 65).

For every patient in the sample it is possible to determine two "ages": first, the age at first admission to the mental hospital, which is the "admission age," and second, the "anniversary age," which becomes the age she would have had to be at admission *if* her oldest child was to be at her age at parent loss. Thus a woman who lost a mother at 10, and was 25 when her first child was born, would have an "anniversary age" of 35. We allow one year in either direction, so her case is a "hit" if she enters the hospital for the first time when 34, 35 or 36. To determine whether observed "hits" differ significantly from chance, we placed all the "admission ages" in one pool, all "anniversary ages" in another, and then established how often in a random selection of one from each pool a coincidence (or near-coincidence as defined by the one year leeway) would occur. This procedure in effect determines whether or not a given patient's own anniversary dates are any better "targets" for her onset of illness than anniversary dates "borrowed" from other patients in the sample. It is submitted that this procedure is more valid than any attempt to match some outside control group to this highly specialized sample of patients.

Two methods have been used to make these determinations. One involved the randomization test developed by Hoeffding (9), the application of which to these data was worked out for us by Dr. Lincoln E. Moses of the Department of Statistics, Stanford University.[7] These results were checked by Dr. Quinn McNemar, also of Stanford University, who evolved a somewhat different and more direct method of making the computations. Dr. McNemar's analyses led to results equivalent to those obtained by Dr. Moses, thus providing an additional confirmation of the general method involved in the evaluation of these data.

[7] We also wish to acknowledge the assistance in consultation of Dr. Charles Stein, Department of Statistics, Stanford University.

TABLE 8

Coincidences Between Patient's Age at Parent Loss and Age of Oldest Child at First Admission of Patient to a Mental Hospital: Women Who Lost Mothers Between the Ages of 2–15

	Father Loss	Mother Loss
Sample Size	82	65
Obtained Coincidences	9	14
Expected Coincidences	10.56	7.98
Standard Error	2.99	2.57
Normal Deviate*	−.35	2.143
Probability (2-sided)*	NS	.032

* Corrected for continuity. Table provided by Lincoln E. Moses.

The results for the women who lost fathers and mothers are presented in Table 8. This picture is clear. The coincidences with the age at death of the father are at chance level. The coincidences with the age at the mother's death are significant at the .032 level, using the conservative two-sided test.

It should be noted that the original data collection involved cases through age 18; the upper limit was later reduced to 15. In the initial proposal, we chose a sufficiently broad band in order to prevent our missing the study of pertinent case material while cases were in the hospital and still available to us. Analysis soon showed, however, that those cases where the parent had been lost at the later ages were less pertinent than those where the parent had been lost earlier, and this because of the greater independence from parents by the ages of 16, 17, and 18. Data collected for the full age range of two through 18 years were, however, subjected to a one-tailed test, and yielded a p-value of 0.047 (one-tailed) for coincidences between the patient's childhood age at mother loss and the age of the oldest child at first admission to the mental hospital. Thus while the use of the less satisfactory age range reduces the significance of the probability index, it nonetheless remains below the five per cent level.

As a further check on possible bias rising

TABLE 9

Coincidences Between Childhood Age at Parent Loss and Age of Oldest Child at First Admission: Women Who Lost Mothers: 1954–55, 1956–57.

	Sample 1954–55	Sample 1956–57	Total Sample
Women Who Lost Mothers Between Ages of 2 and 15	37	28	65
Coincidences	6	8	14
Coincidences (per cent)	16	29	22

out of the reduction of the upper age limit from 18 to 15, the data collected before and after this decision may be scrutinized. Reduction of the upper limit was put into effect after the first two years of the investigation. Dividing the sample into the first two and second two years (Table 9) it is seen that, if anything, there is a somewhat larger per cent of coincidences in the second two years. Hence no perceptible distortion has occurred. Had we been capitalizing on chance factors in making the decision to change, the coincidences should have been less in the later part of the study. Thus we feel that the statistical findings reported in Table 8 are satisfactory.

It is unfortunate that the number of men who conformed to the criteria for inclusion in the experimental sample remained too small for statistical analysis. Only 18 men had experienced father-loss and 17 men mother-loss. However, in terms of our finding that women may have anniversaries when a parent of the same sex has been lost, the data for men may be showing a similar trend. Four of the 18 men with father-loss show coincidences with childhood age at loss, while none of the 17 men with mother-loss show such coincidences.

Cases of double loss of parents during childhood would be expected to be few in number. There are only seven, with one age-coincidence case among them. In this instance the parents were lost through murder and suicide when the patient was 10. The patient's first hospitalization had occurred when her first child was 10 years old; we saw her at the time of her second hospitalization a number of years later when her second child was 10.

DISCUSSION

The study of the anniversary hypothesis by both clinical and statistical methods provides an opportunity to examine the gains made through their simultaneous use in relation to theoretical considerations bearing on dynamic processes basic to the data.

Behind the anniversary hypothesis lies a theory of identification, calling attention to a relatively neglected aspect of this phenomenon: the unconscious identification of parents with their children. Clinicians who work not only with problem children but also with the parents of these children realize the intense emotional involvement of the parents in their children's lives. For example, Szurek, Johnson and collaborators for many years have documented this point in their studies where children and their parents were brought under psychiatric treatment simultaneously (10, 12). In our research we started with a disturbed adult in whose neurosis the role of the child was unconscious. The child showed relatively few problems; the parent was grossly disturbed and did not relate his unhappiness to the presence of the growing child. It has been our experience that clinicians rarely scrutinize this point with the thoroughness that it deserves. Neither has it been adequately taken into account by psychiatric or psychoanalytic theory. The very useful concept of identification of the child with the parent has led to many generalizations, but the closely related concept of identification of the parent with the child has been largely overlooked.

In all probability we are dealing not only with the effect that a death in one generation may have in reviving childhood memories, fantasies and actions, but are also facing a broad spectrum of unconscious reactivation on the part of parents toward their children. When this project was first planned, loss was not restricted to parental death. It included loss of parent through separations such as

divorce and desertion, for today these are a more common source of parental loss in childhood. Early in our study, however, these other losses were rejected for two reasons: 1) Dates were too difficult to establish with accuracy. Parents might have separated several times or for differing lengths of time prior to the final one. 2) Loss by divorce could vary markedly in degree. One divorced parent might continue to see the children; another might see them occasionally; yet another might never see them again. Grief, mourning, and guilt under these conditions present special and complex problems that are in need of basic study.

When we turn to relationships with the generation which preceded our patients rather than that which followed them, we see an affinity for like-sexed identifications between the patients and their parents. Age-coincidences among women who lost mothers are at a significant level, while age-coincidences among women who lost fathers are not. Thus in the choice of a woman's identification with either her mother or her father, we expect identification with her mother to be closer, especially when she herself becomes a mother. It is of interest that among men age-coincidences occurred only among men who lost fathers.

In this paper we have not reported another kind of age-coincidence—that between the parental age at death and the patient's age at first admission to a mental hospital. These data will be treated in a later publication, but a brief mention is in order here. Among the 65 women who lost mothers by death, age data for the mother were available for 61; of these, 12 cases first entered the mental hospital when within a year of the age of the mother at death. Only four of these 12, however, were found among the 14 who had age-coincidences also with a child, so that we are seemingly dealing with overlapping but separable phenomena. Among the 82 women who lost fathers, the age of the father at death was known for 72; of these, three first entered the hospital within a year of the father's age, but only one showed an overlapping age-coincidence with a child. It thus appears that the dynamic connection between parental age at death and patient's age at psychosis holds more frequently when a woman patient has lost a mother than when she has lost a father. Aspects of identification between mother and daughter can be manifested in more than one way. In the present paper we have focused upon the child's age in the present as related to the patient's age at parent loss, but this age-coincidence is but one of the occasions that may trigger an anniversary reaction.

Although in some respects statistical manipulations are quite limiting inasmuch as they require the neglect of a great deal of specific data that emerge through case study, there are positive contributions of the statistical method going beyond its determination of the non-chance nature of the observed events. Its first service is, of course, to demonstrate that we are not capitalizing upon chance or rare correspondences and then rationalizing such correspondences into a theory. This requires a method with appropriate controls, careful sampling, consideration of all data (including what clinical judgment would call false-positives and false-negatives), the use of normal and pathological control cases as required. In this study a normal population sample was used to determine average parent-loss as one necessary control to our large hospital sample, and a within-group method was employed for determining whether age-coincidences exceeded chance. The statistical results bore out the evidence from clinical observations.

The statistical method achieved more than confirmation of the reality of the anniversary relationship. It provided data on *prevalence*, which the study of case material alone could not do, and, as a consequence, led to some refinements of the hypothesis. The study of a few cases, no matter how painstakingly done, could not tell us whether what we were dealing with was rare or typical.

Our statistical investigation showed that the prevalent form of anniversary occurred in women who had lost mothers in childhood. The clinical evidence is clear that there are occasional anniversary cases among women who have lost fathers in childhood, but the statistical data show that such cases are unusual. How does this give us a positive lead? If this type is exceptional and response to mother-loss is more prevalent, we may ask whether, in cases of daughters with father-loss, there has not been a deeply disturbed mother-daughter relationship that has taken precedence over other factors. Going back to our clinical data with this in mind, we do indeed find that the mother's reaction to the loss of the father may be a crucial factor in providing a setting for an anniversary reaction.

Having established by clinical and statistical methods that the anniversary reaction is a demonstrable precipitant in mental illness, how far can we go in making generalizations about anniversary reactions?

Dynamic theories have tended to interpret the psychology of the normal from a study of the psychology of the neurotic without concurrent studies of a normal population. More studies of less disturbed people are clearly needed. We believe, for example, that the study of anniversaries in normal populations will prove fruitful, but we have no way of knowing, from our hospital population alone, what form such anniversaries might take. We would predict, for example, that marriage and divorce, migration from one place to another, choice of occupation, or the development of psychosomatic symptoms will in many cases have anniversary aspects, that is, will show a timing related to events in the individual's childhood reflecting in this generation something that happened in the previous generation. Some of these relationships have been noted by us (8) and by others (1, 13). We hope that our research will encourage tests of the anniversary hypothesis elsewhere, *e.g.*, in out-patient clinics as well as in normal populations. Eventually we would hope to have the anniversary reaction understood as a principle within normal psychology, but we do not think this is possible until after it has been appropriately investigated in representative groups.

SUMMARY

The anniversary hypothesis is stated as follows: If a person has lost a parent by death in childhood, and that person subsequently marries, has children, and later becomes hospitalized for the first time for mental illness, the first hospitalization is likely to occur beyond chance expectance when the oldest child of that person is within one year of the age the person was when the parent died.

Evidence bearing on this hypothesis was obtained by searching the records of 8,680 patients entering a State Hospital during nine months of each of the years 1954–57. Of these patients those were retained who were native white, under 50, married and with children at the time of first admission, with a diagnosis of psychotic or psychoneurotic (omitting alcoholics, addiction cases, organic cases, and those diagnosed as psychopathic) and with childhood loss of a parent between the ages of two and 15 years inclusive. The resulting sample of 193 (40 men and 153 women) was very similar in educational and occupational level and religion to the rest of the hospital population.

A statistical test of the anniversary hypothesis was made with the sample of 82 women who had lost fathers and 65 who had lost mothers. The anniversary hypothesis was not confirmed for women who lost fathers, the age-coincidences being only at a rate expected by chance. For women who lost mothers, however, there was a significantly greater occurrence of age-coincidences than would be expected by chance, the appropriate test yielding a p-value of .032 (two-sided test). Thus a non-random factor in agreement with the anniversary hypothesis was demonstrated.

Because the precipitating factor that set

the target date for the onset of mental illness was the death of a parent, the appropriate interpretation is that something functionally related to this historical trauma is important in the etiology of the mental illness. Because the diagnostic categories were primarily psychotic, chiefly schizophrenic, a functional contribution to these illnesses is indicated. A demonstration of a functional etiological factor in schizophrenia does not deny that hereditary susceptibility may also be present, but it suggests that we cannot afford to neglect a search for functional etiological factors.

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According to recent research prolonged sensory deprivation in human adults sometimes yields unexpected and bizarre consequences. The theoretical and practical implications of this work have enhanced its interest to many workers in the social and behavioral fields. The articles which follow have been adapted from a symposium held at the annual convention of the American Psychological Association in Cincinnati, September, 1959. The symposium was conceived as an attempt to air certain viewpoints on the relation of the new findings to existing knowledge and to gain a much needed perspective thereby. Each of the papers presents the systematic approach of its author to the issues raised.

Dr. Richard Held, of Brandeis University, was the Chairman and presents one of the papers. Dr. Donald O. Hebb of McGill is the discussant. Other participants are Dr. Sanford J. Freedman, of Harvard, Dr. Austin H. Riesen, of The University of Chicago, and Dr. Hans-Lukas Teuber, of New York University. JNMD takes pleasure in publishing this symposium and feels that it will be of especial interest to its readers.

The Editors

SENSORY DEPRIVATION: FACTS IN SEARCH OF A THEORY

Perceptual Changes in Sensory Deprivation: Suggestions for a Conative Theory[1]

SANFORD J. FREEDMAN, PH.D.[2]

(22)

Since the original work of Dr. Hebb and his students (2, 11) on the sensory and social isolation of human beings, a number of studies have been reported under the rubric of "sensory deprivation." These experiments have involved restriction or occlusion of visual, auditory, and kinesthetic inputs. Usually (1, 3, 6), the subject lies on a bed in a cubicle or a small room (or even in a respirator (22)) wearing translucent goggles to produce a diffuse visual field and with earphones or some other device to supply a continuous masking noise. Cardboard cuffs on the forearms and heavy cotton gloves are worn to reduce tactile stimulation. He is instructed to move little or not at all, and remains in this situation without interpersonal contact for hours or days. The most striking finding in these reports taken as a whole is that radical changes in functioning occur under what appear to be relatively innocuous conditions. A few of the published reports concentrate upon perceptual changes, but most restrict themselves to changes in cognitive functions, namely: hallucinations, delusions, depersonalization, or changes in the body image.

For the purposes of this symposium, I should like to separate the two kinds of effects, discussing perceptual disturbances only. It is not possible to make any statement about the nature or occurrence of such disturbances in experiments where no perceptual tests were made although I think it *is* generally assumed that they are to some extent common to almost any kind of sensory deprivation.

It will be helpful briefly to review the findings of several experimenters who have described in detail and attempted to measure visual disturbance. At McGill, Doane (4) isolated 13 subjects for from two to five days, and shortly thereafter Heron, Doane, and Scott (11) isolated each other for periods of

[1] This research was supported in part by the U. S. Air Force under Contract No. AF33(616)-5663 monitored by Aerospace Medical Laboratory, Wright-Patterson Air Force Base, Ohio.

[2] Massachusetts Mental Health Center.

six days each. Immediately following the experiment, a variety of remarkable disturbances was described. Although these were largely dissipated in about 20 minutes, some effects persisted for over 24 hours. There was apparent movement independent of movement by the observer; people and the room itself seemed to change shape and size, with walls bulging forward and back. An ash tray or a watch appeared to expand or contract. There were distortions of shape with a tendency for straight edges or lines to appear curved and for the region about the fixation point of flat surfaces to appear to bulge outwards. Pronounced negative after-images appeared and colors seemed bright, highly saturated or luminescent. Black figures on a white ground gave rise to subjective colors. Finally, there were consistent changes in size constancy.

In our laboratory, under similar isolation conditions for uninterrupted sessions of only eight hours, 20 subjects reported many comparable (though, of course, less extensive) effects (5).[3] These persisted, on the average, about five minutes: there was shimmering, instability, undulation, and fluctuation of contours in the visual field with apparent changes in the size and shape of simple forms. Shapes seemed to flow in a direction away from the fixation point. Ambiguous figures alternated more rapidly and the apparent speed of a moving contour decreased markedly. Most of these effects have recently been replicated by Courtney[4] at the Boston City Hospital, also in short sessions.

Now, what is going on in these experiments; how can we explain such effects? There seems to be a partial breakdown of the customary process of stabilizing, structuring and organizing the visual world. The normal adult organism structures his perceptual world in accord with an overall spatial schema depending upon basic coordinates such as up-down and self-other. Transactions with the environment require adequate localization and stabilization of self and objects in the spatial schema. It would seem that in sensory deprivation, this schema has somehow become disorganized, degraded, or to use Werner's word "de-differentiated" (21). The terms "schema" and "spatial schema" are here employed in a specific sense directly analogous to the body-schema concept of Henry Head and the English school of neurology. Head (8) defined the body-schema as an integrate of all sensations, perceptions, memories and images continuously being built up and altered by new activity and new stimulation and also being used continually for reference in locating any new sensation and in correctly initiating any new movement. The spatial schema is a very similar integrate, a dynamic psychic structure. Neurologists have found it more and more necessary to link the schema of the body to a schema of the world of external objects (17).

Since bodily and spatial schemata are so intimately related, alteration of one may produce changes in the other and perhaps any subdivision is arbitrary. The conditions of reported sensory deprivation experiments have drastically curtailed or modified cues for the maintenance of both schemata so that the effects are confounded. Feelings of depersonalization, specific changes in the body image, and paranoid fears which have been described may all be related to deterioration of the body schema. However, since we have found no significant correlation in 57 subjects between the occurrence of these kinds of effects and visual disturbance (5), it may be permissible to disregard the former temporarily. We would attribute the visual disturbances to deterioration or degradation of the spatial schema resulting from particular kinds of prolonged sensory stimulation whereas cognitive effects are probably more closely related to social isolation, inability to test reality, personality differences, and the total unstructured situation.

[3] This experiment is described in detail elsewhere (Ref. 5; also in: *U. S. Armed Forces Med. J.*, November and December 1960, **11**: 1330–1348 and 1479–1497.

[4] Courtney, J. Personal communication, August, 1959.

The overall spatial schema may be subdivided into lower-order schemata for vision, audition and kinesthesis which are normally well-integrated. Such schemata, as well as their integration, are not innate givens, but have to be slowly learned by the infant organism. Riesen has shown that interference with this ontogenetic development by subjecting new-born monkeys and kittens to deficient sensory environments for prolonged periods makes subsequent development of effective schemata difficult and perhaps impossible (14, 15, 16). Senden's review of the literature (18) on the vision of the congenitally blind given sight by surgical operation leads to the same conclusion. These reports suggest that spatial orientation develops by repeated observation and testing of ordered relationships in the perceptual environment.

Spatial schemata are not only originally learned, but may be modified by new learning. The classic disarrangement experiment of Stratton (19), the work of Kohler (12), and more recent studies by Held (9, 10) transform the visual environment of the subject by *systematically* shifting certain sets of cues. However, the new sensory field is isomorphic with the old one and, furthermore, auditory and kinesthetic sub-schemata are not directly impaired. In the normal relationship of an adult human to his environment, "up" is always "up" and "down" is always "down." If he should wear inverting prisms, "up" becomes "down" and "down" becomes "up." This reversal continues becoming the basis for learning a whole new spatial order. Other relationships in the field are relatively undisturbed. Objects which had previously been contiguous, for example, remain so and distances between familiar objects are not appreciably altered. The subject must modify his visual schema and re-integrate it with other schemata if he is in any way to function effectively with the new perceptual environment. He is able to do so by incorporating the new set of constant relationships which has been provided. Whether "up" is always "up" or is always "down" is relatively unimportant provided that the relationship be *continuous and predictable*.

In sensory deprivation, rather than a systematically transformed environment, the subject is presented with an undifferentiated, homogeneous sensory environment which provides no useful information for spatial orientation. It is unreasonable to think that simple forgetting of greatly overlearned schemata occurs in such a short time as eight hours or even a few days under these conditions. After all, a good night's sleep does not produce marked visual disturbance. No, the answer is not one of decay over time. We are forced to postulate an active process which interferes with and degrades preexisting schemata. Since they are flexible and modifiable, and since appropriate schemata are biologically necessary, the organism must continuously (albeit unconsciously) sample his perceptual environment both for reaffirmation of existing schemata and for evidence of change. The process of sampling and of incorporating observed, ordered relationships into a personal perceptual schema is one of extracting useful information from the welter of "noisy" sensations with which the organism is constantly bombarded. This activity may be called adaptation, accommodation, or reality-testing.

In sensory deprivation experiments, the extrinsic visual field is homogenous and, as noted above, an undifferentiated field provides no usable information for spatial orientation. In addition, intrinsic factors complicate the situation. After adaptation, spontaneous random firing of retinal ganglion cells would dominate the neuro-visual system. In the language of communications theory, this is a "noisy" situation. Greenblatt and I have compared the effects of prolonged exposure to strong diffuse light with blackout conditions (5). Subjects experience visual disturbances in either situation despite apparently large differences in quality and quantity of stimulation. However,

the two conditions may be considered as roughly equivalent for the visual nervous system since both are steady states to which there is rapid adaptation. The visual receptors respond predominantly to changes of state and not to steady states (7). Once adaptation to any homogeneous field has occurred, the system may come to be dominated by the spontaneous, non-patterned (i.e., "noisy" or random) discharge of the retinal ganglion cells. In fact, Granit (7) and Kuffler (13) have each reported that such spontaneous activity is greater in the dark-adapted than in the light-adapted eye (cats and frogs).

Held has postulated that noise is *per se* disorienting. Following up this notion, he and I recently conducted an experiment where extrinsic visual "noise" was supplied to the subject in sensory deprivation (*Percept. Mot. Skills*, **11:** 277–280, 1960). The effects were compared with those of diffuse and blackout fields. Through translucent goggles, the subject viewed a rapidly changing visual field established by a set of incandescent lights of different intensities flashing on and off in random sequence and duration (maximum intensity 250W, maximum period on for any single light = 2 sec.). As predicted, this condition produced similar but far greater changes in apparent visual speed than did homogeneous stimulation.

The fact that perceptual disturbance occurs both with diffuse light and with blackout conditions as well as with flashing lights, indicates that it is the absence of order and/or meaning, the lack of usable information, that tends to degrade the spatial schemata. Riesen has reported several experiments (15, 16) where dark-reared animals (chimpanzees and cats) were exposed to diffuse light daily. This stimulation did not lead to normal visual function; patterned vision seemed to be essential, as with the sensory deprivation experiments, in the building of appropriate schemata for spatial orientation. With an adult animal or human being, the schemata would be highly over-learned and, as noted above, it is not reasonable to think that they would decay or be forgotten in the absence of reenforcement over a short period of time. The explanation must be in terms of an active process in the waking state, a conative process. The organism strives or seeks continuously and automatically to find ordered relationships in the perceptual environment. When, in the experiment on sensory deprivation, the subject is presented with a disordered perceptual environment, his previously acquired spatial schema becomes useless. What is worse, it becomes impossible to modify the old schema or to develop a new one since he cannot extract from the stimulus field the amount of information required to maintain any spatial schema at all.

But, spatial orientation is vital to the organism. If it didn't make any difference, adaptation would never occur in the disarrangement experiments. Since spatial orientation is essential, striving for it continues automatically even when relevant cues are absent. The individual keeps sampling his environment and, finding only noise, tries to accommodate to the noise.

We would postulate that it is this process of seeking orderly relationships where there are none, and of unconsciously striving to incorporate non-order into previously existing schemata which degrades the perceptual frame of reference. The properly functioning organism imposes structure on a fluid environment according to a learned set of relationships which have proven dependable and useful. Such a structure is not necessarily the same for various forms of life but must be related to the needs of the organism and sufficiently congruent with important aspects of the external world to afford satisfaction of those needs (20). Perception is thus always selective and always testing the cues that are essential to the maintenance of various schemata. With the breakdown of the internalized frame of reference, it becomes increasingly more difficult to structure the environment, to impose constancies and stabilities on the perceived visual world. As

a result, contours fluctuate, objects seem to change their size and shape, subjective colors appear, and walls bulge in and out. Renewed experience with a patterned environment is then required before normal visual function can be restored.

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Studying Perceptual Development Using the Technique of Sensory Deprivation[1]

AUSTIN H. RIESEN, PH.D.[2]

(52)

Sensory deprivation of the newborn (or newly hatched) organism has been used as a special condition for the study of perceptual and cognitive arrest which may be correlated with limited opportunities for certain interactions between the developing organism

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and the environment. Reviews of experimental and case-history evidence are available (*cf.* 2). Specific results and general conclusions vary with the organism and with the nature of the response indicator employed. The degree, kind, or duration of deprivation can each in turn be shown to have significant effects with some organisms on some indicators. To what extent the results with this procedure represent simple arrest of development or, on the other hand, reflect both initial arrest and a superimposed regressive or atrophic process remains to be more adequately determined. That both types of effect do occur and that they can be separated there is no longer any doubt.

The developmental approach to visual perception, with or without the use of the technique of visual deprivation, has clearly demonstrated innate neural organizations which govern reflex responses in higher mammalian species. One class of reflexes depends upon changes in the intensity of visual stimulation, and another is related to changes in the distribution of light across the retinal mosaic. Sudden alterations of light intensity produce pupillary and palpebral reflexes, and quick movements of the head and neck which appear and disappear again at certain maturational stages (*cf.* 10), showing little dependence upon prior conditions of sensory deprivation. Optokinetic responses and associated head rotations depend upon the gradual and continuous shift of sharp light gradients across the retina, and are easily demonstrated after complete visual deprivation.

In our laboratories (1, 7–9) we have been unable to demonstrate that any of the reflexive behavior that is seen after visual deprivation will directly support locomotion, manipulation, or the finding and eating of food in primates or in cats. There is such evidence, however, for lower mammals such as the rat and for birds. In the present paper further evidence will be presented in support of the hypothesis that with encephalization of the visual sensory system visually guided behavior develops in ontogeny as a function of patterned visual stimulation.

An earlier literature on normally reared cats tends to imply that visual placing is a reflex that matures with the growing nervous system. Warkentin and Smith (11) placed the mean age of first visual placing, the extension of the forelimbs before bodily contact when the animal is brought toward a surface, at 25 days, with a range of 22 to 28 days. These authors also reported tests of visual acuity as measured by the optokinetic response to a moving striated visual field. By day 25 the kittens were responding to stripes so narrow as to subtend only 11 minutes of visual angle.

Since tactile placing appears quite early, between days 11 and 16, there is no lack of ability to make the limb response involved, and the hypothesis strongly suggested is that visual placing depends, following adequate development of the sensory and effector units, upon the visuo-motor learning that is normally supported by an interaction with the visual environment during days 15 to 25.

In our experiments (5) six litters of kittens were used. One or two members of each litter were control animals, the other two or three serving as experimentals. There were 15 experimental subjects and 10 in the control group. Each mother and her kittens remained in normal indoor lighting of the laboratory until the kittens were 18 days of age.[3] They were then put into a darkroom for 23 hours per day. During the other hour they were in light, the control animals without visual restriction, the experimental kittens wearing hoods made of fine percale which diffused the light. Sufficient air for breathing also passed through the cloth, and the kittens showed no signs of discomfort. All kittens were handled an equal amount each day.

The unhooded kittens were tested for visual placing at the beginning and end of each hour in the light. The hooded kittens were carried through the same movements to

[3] Each kitten's eyes had opened by 10 days from birth.

further equalize handling. Support of the kitten was given through the body and hind legs, with the head and fore legs free. With a slow even motion the kitten was carried forward and slightly downward toward the edge of the table used as the testing surface. If the fore legs were extended toward the table before any part of the body, limbs, or vibrissae touched it the animal was scored as capable of visual placing. On the day following the first appearance of visual placing in an unhooded kitten its litter mates were left unhooded during the hour of light and testing continued as before. For the sixth litter to be carried through this procedure the method was altered so that the mother and kittens remained in the light continuously until the response appeared, testing being done at hourly intervals.

The unhooded (control) kittens first gave placing responses to the visually perceived surface at age 27, 28, or 29 days. No experimental kitten showed the response when the hood was first removed. Remarkable consistency was shown in that each of the 15 animals of this condition gave its first placing response after five hours in the patterned light. This was true whether the five hours accrued one hour per day or continuously.

During the second unhooded hour the experimental kittens typically spent considerable time sitting straight up and still, staring fixedly at the mother's face. This was one of the first indications of a gradually increasing use of vision that followed.

In other experiments, kittens and primates whose early visual experience was complete with the usual room patterns, but who were not permitted to move freely during such visual exposure, failed to develop either the visual placing or the protective eye blink (1, 7). These animals did, however, show normal binocular fixation and visual pursuit of other animals or persons moving about in the room.

One may raise the question of whether or not the initial visuo-motor reflexes support in some degree the development of the eye-paw or eye-hand coordinations that come about only with motor activity engaged in while the animal is seeing what he approaches. To answer this question is not easy. There is no procedure that will enable us to study the development of the learned coordinations in the absence of the prior reflexes. One alternative approach devised by a graduate student in our laboratories for his doctoral research gives at least a first analytical lever for prying apart the mutual interactions of these two factors.

Harding Bishop (3) has completed experimentation in which kittens were raised while seeing the environment through one eye only, the vision in that eye being either upright or inverted (not with right and left reversed, as in some lens experiments done with human subjects). After these animals, under one or the other condition continuously, achieved plateau levels of performance on a variety of mazes, obstacle courses, and an eye-paw coordination test display, the conditions for each group were interchanged. Re-adaptation was then followed for a further series of tests lasting several more weeks.

In this experiment the obvious question is: Do the conditions permit equal levels of performance to be achieved in equal periods of time? Innate reflexes would compete with development under the inverted condition. This is particularly clear in the case of pursuit head movements. A downward or upward pursuit movement of the head causes an increased speed of movement of the image projected on the retina when inverting lenses are worn. Without inverting lenses the pursuit reflex tends to slow down or stop movement of the image, *i.e.*, it aids fixation. So long as the head does not move up or down, this competition does not occur.

Bishop's results show a surprisingly small difference between performance under the two conditions. Only with pooled data from the test battery does the difference reach significance. The fact of a difference supports the conclusion that the reflexes have some-

and the environment. Reviews of experimental and case-history evidence are available (*cf.* 2). Specific results and general conclusions vary with the organism and with the nature of the response indicator employed. The degree, kind, or duration of deprivation can each in turn be shown to have significant effects with some organisms on some indicators. To what extent the results with this procedure represent simple arrest of development or, on the other hand, reflect both initial arrest and a superimposed regressive or atrophic process remains to be more adequately determined. That both types of effect do occur and that they can be separated there is no longer any doubt.

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Bishop's results show a surprisingly small difference between performance under the two conditions. Only with pooled data from the test battery does the difference reach significance. The fact of a difference supports the conclusion that the reflexes have some-

thing to do with the development of visual-motor coordination. Their rather mild effect emphasizes the important contribution of the learning factor.

Other observations show that the animals we have deprived of vision during initial growth periods must progress thereafter through stages of learning to fixate and pursue moving objects, learning to avoid obstacles, find food, discriminate movement from non-movement and so on. Many of these data fit into a stimulus-response conception of the learning process.

When intensity cues are all that need to be discriminated the animals learn as quickly whether or not they have undergone visual deprivation prior to the commencement of discrimination learning (8). When complex patterns or movements must be discriminated it is obvious that the pattern-deprived animals *begin* the training procedure at a great disadvantage. To me this means that a sensory-sensory (S → S) associative integration has preceded the formal training in the pattern-experienced group. I am quite willing to call this cognitive learning, as many others have done along with Freedman, and with Tolman before him.

We still need more data on the significance *during* the early patterned light experience of active movement (as opposed to passive movement) of the animal or its appendages. Held has effectively demonstrated that this can be crucial to ego-centric localization in re-adaptation following disarrangement of the visual field. Our study of visual movement discrimination (8) showed this kind of activity (locomotion and/or manipulation) to be essential during perceptual development if visual motion is to be discriminated from non-motion. Body postures and movement and the stimulation consequent to the movement are entering into a kind of response → stimulus learning. The reafferentation principle, as extended to perceptual disarrangement problems by Held, calls for the inversion of our customary S → R formula.

So, by this point I am asserting that we need a theory broad enough to include S → R, S → S, and R → S learning in any complete formulation of perceptual development.

I am not quite sure whether the perceptual changes reported for human subjects following eight to 48 hours of visual deprivation include a loss of learned integrations. Perhaps something new develops during those hours of "random" input which overlays and competes with previously learned cell assemblies. If so, longer periods should produce greater and more persistent effects.

I can report in this connection two experiments, one done with a monkey which was deprived of visual stimulation from the age of five months to the age of 10 months, the other on a chimpanzee deprived of patterned light from 10 to 18 months of age.

Both animals showed loss of recognition for food or food containers. In each case approximately eight days were required before these animals regained the clear discriminations between pieces of food and inedible objects which they had shown with complete accuracy before the deprivation period.

Both animals showed inaccuracies of distance and direction when making protective eyelid responses and when approaching and reaching for objects. These improved rapidly during the first three days post-deprivation.

During initial hours of return to the visually structured environment these animals showed, in addition to the eye-hand inaccuracies, some peculiarities of eye movement and binocular coordinations. A pendular nystagmus of rapid (8 per sec.) rate appeared in the chimpanzee youngster and moments of exotropia or divergent squint appeared briefly but repeatedly. The monkey showed occasional eye and head oscillations of a more jerky pattern, *i.e.*, faster in one direction than the other. These were transitory and in a matter of a few hours disappeared.

When we saw these incoordinations we were immediately struck by their similarity to what we commonly see in the animals deprived from birth. Infant primates and

pattern-deprived kittens take considerably longer, however, than did these two animals to develop steadiness of gaze and the ability to fixate binocularly. Once ocular coordinations are established, they can be regained after disuse with a large savings, as is true of learned behavior generally.

What theoretical implication is inherent in these motor incoordinations? Identical stimulating conditions are not producing the same responses, it is true. However, I do not think that an S → R formulation can, by itself, account for the data. Much of the loss in fixation skill (both monocular and binocular) must be due to a breakdown of coordination within the motor system itself. This can be conceived of as a decomposition of rather fragile response → response associations. Charles Osgood earlier suggested in his *Experimental Psychology* (6, p. 401) that the term *motor cell assemblies* be applied to integrations between motor units. Eye fixation skills, and I think perhaps also eye-hand coordinations, must involve the development and maintenance of cell assemblies *within* and *between* motor nuclei. In the absence of patterned visual input, autonomous rhythmic activity in these centers appears to gain the upper hand.

In summary, to account for the prolonged refinement of the perceptual control over behavior that higher mammals exhibit in their discriminations in space we need a considerable extension of learning theory. The testing of this theory will involve a much more refined analysis of the neurophysiological and behavioral units which enter into the finished performance. When these units are more adequately isolated experimentally, necessary conditions for their associative integration will be found. I predict that they will be classifiable into S → S, R → R, and R → S groupings in addition to the favorite category of recent learning theories, the S → R bonds.

W. K. Estes (4) has shown us that S → R units may become associated on an all-or-none basis. I suspect that this may some day prove to be true also for the three additional types of unit that I am proposing here to account for effects of sensory deprivation. We need empirical grounding for these views. The chief obstacle standing in our way remains that of studying the behavior of such units in isolation.

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Exposure-History as a Factor in Maintaining Stability of Perception and Coordination

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Stimulus deprivation degrades coordination and perception in higher animals and thus strikingly demonstrates the importance of exposure-history in spatial coordination and perception. But deprivation is by no means the only method of demonstrating the influence of past exposure on current achievement. The technique of rearrangement—the procedure associated with Stratton's classic experiment on re-inverting the retinal image (15)—produces results resembling in many ways those following deprivation and provides a useful technique for studying the influence of exposure-history. The techniques of rearrangement and of deprivation have produced evidence of an underlying process that may account for the effects of both.

DEPRIVATION AND REARRANGEMENT

Reports from McGill University (2, 10) summarize the perceptual aftereffects produced by deprivation in human adults—the losses of object constancy, the apparent movements accompanying bodily movements, the shape distortions and the color anomalies. Ivo Kohler's monograph (12) presents the most striking results to be found in the extensive literature on rearrangement experiments. These experiments preceded and followed Stratton's work and total several dozen in number including a series we have done in our laboratory (3–7). Comparisons among the reported results show that most of the aftereffects of deprivation can be matched by similar aftereffects following rearrangement procedures. But one very crucial difference shows up in these comparisons. The aftereffects of deprivation are variable, inconsistent and unstable, whereas the aftereffects of rearrangement are relatively constant, consistent and stable. Consider a comparison example involving apparent movement as an aftereffect. In the McGill laboratory Heron, Doane, and Scott's Subject B reported after deprivation, "The wall is moving all over the place. . . the visual field is in such a state of chaos" (10). A subject who wears wedge prisms that laterally displace the visual field will at first report that all objects move when he does but after days or weeks of this rearrangement he will no longer report this apparent movement. A subject of Kohler reported, on removal of the prisms, an aftereffect evidenced whenever he moved towards an object. The object appeared to move in one direction and stopped moving when he stopped moving. The disorderly aftereffect following deprivation contrasts sharply with the orderly but related aftereffect of rearrangement.

On the other hand, the periods of exposure required to develop the aftereffects of deprivation and rearrangement are of a similar order of magnitude. In a matter of hours deprivation produces substantial aftereffects. In our laboratory we have shown that full compensation for errors of directional localization also occurs in a matter of hours (4). All of these aftereffects seem to imply that habits practiced for a lifetime are extinguished or forgotten in a relatively short period of time. This rapid loss of old habits or at least the appearance of such loss under apparently innocuous conditions poses a serious problem for theories of learning.

DISARRANGEMENT AND NOISE

A new variation on an old rearrangement experiment allows us to pursue the comparison between rearrangement and deprivation. Helmholtz was the first to show that if a subject viewed his moving hand through a

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wedge prism that displaced the retinal image to the left, for example, he would on removal of the prism predictably err to the right in reaching for objects (9). This error represents compensation for the formerly-present prism. In one of our recent experiments the subject viewed his hand through a prism that slowly but continuously varied in power (1). As a result the image of his hand was continuously displaced through a range of positions so that over a period of time the position of the image was ambiguous. The prism axis was set to produce large displacements in the horizontal or left-right dimension and much smaller displacements (about 5 per cent of the horizontal) in the vertical dimension. The ambiguity of position was therefore mainly in the horizontal dimension. At the termination of 30 to 60 minutes of exposure, measurements of the subject's hand-eye coordination showed greatly increased dispersion in the horizontal dimension as compared to that of the vertical dimension. This result shows that a form of rearrangement (one that we prefer to call a disarrangement) can, after a relatively short exposure period, disrupt or degrade a habitual and stable function. This aftereffect closely resembles a consequence of deprivation and the conditions for its generation could represent a model for producing deprivation effects, except for one apparent discrepancy. In our experiment there is a deliberately contrived ambiguity in the conditions of exposure, whereas no similar ambiguity is apparent in the exposure conditions of deprivation. To bridge this gap we will introduce an assumption that has a factual basis in neurophysiology.

There is ample evidence that some neurons fire spontaneously in the absence of their usual triggering energy and that members of a group of such neurons will fire in a non-synchronized or random manner. In the absence of systematic external stimuli, an "alert" sensory-neural system may be dominated by such internally generated noise (the word *noise* is used in analogy to acoustic noise—sound having energy distributed over a large range of audible frequencies). Noise—and this is the core of the assumption—may constitute a source of ambiguities of the type our experiment imposed upon the visual-motor system by external means. In other words, to pursue the above example, visual noise might produce an ambiguity analogous to that produced by the variable positioning of the image of the hand on the retina. With the aid of this assumption we can predict that the mere absence of orderly visual stimulation can degrade visual-motor coordination. The apparently innocuous condition of deprivation is really lethal because it allows intrinsic disorder to disrupt the internal state or schema that is responsible for ordered coordination.

In support of this assumption I want to mention another experiment done in our laboratory but this time not using disarrangement. The experiment shows that one consequence of deprivation can be simulated by direct exposure to a noisy field of vision. After deprivation the McGill subjects reported that a moving contour seemed to show "perceptual lag" (10). When objects were moved across the visual field, part of the moving figure appeared to trail behind the rest. Following this lead, we developed a device to measure the apparent speed of a moving contour and demonstrated that this apparent speed is markedly reduced by prolonged exposure to a noisy visual display (8). The display was produced on a television picture tube by a special electronic circuit. With the help of Sanford Freedman we showed that the same effect occurred in an eight-hour sensory deprivation experiment. In addition, prolonged exposure to a completely static field—one containing contours but devoid even of the amount of object movement that occurs in normal scenes—increased apparent visual speed. These results provide independent support for the assumption that noise plays an important role in producing the effects of deprivation.

ADULT REARRANGEMENT AND NEONATAL DEPRIVATION

Up to this point we have discussed the effects of adult or post-developmental deprivation. Although neonatal deprivation produces deficits of coordination and discrimination similar, if not equal in magnitude, it is neither obvious why this should be so nor that there is a common process underlying both forms of deprivation. However, a comparison of selected results of neonatal deprivation with those of rearrangement is instructive in this connection. The comparison will point to a common factor necessary for both the development of coordination in the neonate and compensation for rearrangement in the adult.

In a series of experiments we have demonstrated that compensation for various optically-produced rearrangements requires *re-afferent* stimulation. Re-afferent stimulation is stimulation that changes as a consequence of the natural movements of the recipient organism. The term *re-afferent* was coined by the German physiologist v. Holst to distinguish this form of afference (activity in sensory nerves) from ex-afference resulting from stimulation not dependent upon movements of the recipient organism (11). To demonstrate the crucial role of re-afferent stimulation we exposed subjects to rearrangement under two conditions of movement. Other relevant conditions were kept as near to equivalence as possible. In the first or active condition the subjects either locomote or simply move a limb. In the second or passive condition the subjects, who remain alert, receptive, but passive, are moved by the experimenter in a manner equivalent to that of the active condition. The active subjects almost invariably compensate for the errors induced by rearrangement; the passive subjects do not. This result has been obtained in studies of several different conditions of rearrangement (4, 6, 7). In addition, the same result was obtained in the disarrangement experiment reported above (1). For example, subjects who wear prisms and walk about in their normal environments show shifts in ego-centric localization ranging up to full compensation for the prism-induced error after many hours of exposure. However, the same subjects moved over the same path in a wheelchair showed no compensation (4). The same displacements of the head and body and attendant visual stimulation were produced under both active and passive conditions but only the active subjects showed shifts in localization.

Our demonstration of the crucial role of movement in compensation for rearrangement closely parallels Riesen's demonstration of the importance of bodily movement for the development of coordination in the neonate (4). Riesen has long studied the deficits of visual-spatial performance that follow visual deprivation in neonatal mammals. Recently he has demonstrated similar deficits in animals who were merely restrained in holders. Although reared with visual stimulation from nearby objects they were deprived of the opportunity for gross bodily movement. We believe these results constitute an almost sure demonstration that re-afferent stimulation is as crucial in the development of coordination as in compensation for rearrangement. Moreover, they support our view that the rearrangement experiment taps a process that operates in the development of spatial coordination. Our conclusion concerning neonatal development is of course that such development requires re-afferent stimulation for the same reasons as does compensation for rearrangement. Its absence precludes appropriate development of coordination and spatial perception.

COMMON CONDITIONS

We have discussed resemblances among the effects of adult and neonatal deprivation, rearrangement, and disarrangement. In summing up the factors common to all these effects, the prolonged and atypical exposure-history must first be mentioned. By atypical

we mean that contacts with the sense-stimulating properties of the environment deviate from those normally available to the organism. In particular, we have emphasized the importance of atypical re-afferent stimulation. This form of feedback depends upon the properties of the objects and upon the media of transmission intervening between action produced by contraction of skeletal musculature and causally-dependent sensory stimulation. For example, when a viewer moves his head the images of whatever objects are focused on his retina will be displaced by an amount that depends upon their direction and distance from his eye and upon the light-transmitting media intervening between object and eye. The feedback loops that include stable parts of the environment will, in general as a result of the geometry and kinematics of movement, cause a given movement to yield a characteristic re-afferent stimulation. The combinations of movement and fedback stimulation are bound to be replicated with increasing duration of exposure in a stable environment. We have claimed that this source of ordered contact with the environment is responsible for the stability, under typical conditions, of visual-spatial performance (4).

Rearrangement produces atypical relations between movement and contingent re-afferent stimulation. But the relations remain just as univocal—each movement has its unique train of re-afferent stimulation—as under typical conditions. Progressive shifts in coordination compensate for the errors induced by these atypical conditions. Clearly, the compensation achieved in rearrangement reflects the attainment of increasingly precise and stable functioning for the rearranged condition. As a corollary, return to the normal condition that preceded rearrangement entails errors similar to but in the opposite direction to those initially induced by the rearrangement. On the other hand, experimental procedures (disarrangement) that make ambiguous the relation between movement and re-afferent stimulation will progressively degrade coordination by making it imprecise under normal circumstances. For if the same movement has several different re-afferent consequences, and different movements have the same consequence, then there will be a loss of discrimination when normal conditions are regained.

A COMMON PROCESS

Our analysis of the conditions that generate the effects of deprivation, rearrangement, and disarrangement suggests a provisional neural model to account for these effects. Von Holst (11) and others have pointed out the importance of re-afferent stimulation in spatial coordination (see the diagram and discussion in the paper of H.-L. Teuber in this symposium). Their studies imply that some part of the central nervous system monitors efferent signals (labeled "corollary discharge" in Teuber's diagram) activating skeletal musculature. The monitored signal is then summated with the slightly delayed re-afferent signal coming from sensory receptors stimulated as a consequence of the bodily movement induced by the activated musculature. The outcome of this summation is a signal in the central nervous system said to determine perception and further performance. For example, the summation will yield different signals for a displacement of the retinal image produced by rotation of the eyeball as compared to the same displacement produced by movement of an external object. Displacement of the image following contractions of the extra-ocular muscles that produce eyeball rotation is presumed to be accompanied by a monitored efferent signal of magnitude and sign just sufficient to cancel out the re-afferent signal. On the other hand, when displacement of the retinal image is produced by the movement of an external object the resulting visual stimulation—now ex-afferent—is *not* canceled out by a monitored efferent signal. The important ability of

organisms to distinguish moving from resting objects under conditions yielding the same retinal stimulation—the perceptual phenomenon called constancy of object position—depends upon such differences.

In the feedback process described by von Holst a single cycle, starting with an efferent signal and ending with the adjustive consequence of the re-afferent signal, occurs within a fraction of a second. Sequences of such cycles may of course reverberate for many times this duration. In contrast to this rapid adjustment, we have demonstrated in human coordination—and it is probably true of the coordination of many higher mammals (4)—that effects of far greater duration depend upon the same relation of response-produced movement to consequent re-afferent stimulation. For example, we have previously mentioned that to a prism wearer objects appear to move when he moves. This apparent motion is eliminated by exposure of the type that we have repeatedly shown must involve re-afferent stimulation.[2] These findings imply that the combinations of monitored efferent and re-afferent signals available to the nervous system are not merely summated in the manner described by von Holst. In addition they must be correlated and stored in the central nervous systems of those organisms that respond adaptively to novel combinations of these signals.

The diagram of Figure 1 schematizes the process that we suppose to occur in these organisms. The diagram is derived from the Holstian model by adding the Correlation Storage and making a few other modifications. The model is not restricted to eye movements; the skeletal muscle at the origin of the External Loop represents any motor system that can be a source of re-afferent visual stimulation. Instead of assuming a summation between monitored efferent and re-afferent signals, we assume that the re-afferent signal is compared (in the Comparator) with a signal selected from the Correlation Storage by the monitored efferent signal. The Correlation Storage acts as a kind of memory which retains traces of previous combinations of concurrent efferent and re-afferent signals. The currently monitored efferent signal is presumed to select the trace combination containing the identical efferent part and to activate the re-afferent trace combined with it. The resulting revived re-afferent signal is sent to the Comparator for comparison with the current re-afferent signal. The outcome of this comparison determines further performance.

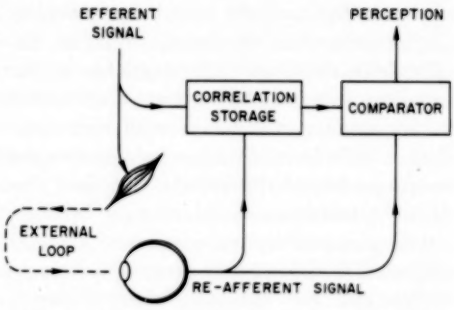

FIG. 1. Schematized process assumed to underlie the consequences of rearrangement, disarrangement and deprivation.

Evidence for progressive adaption to rearrangement implies that the selection from storage by the currently monitored efferent signal must be weighted by the recency of trace combinations when alternatives are available. Thus, for example, if the conditions that make for typical combinations of signals are systematically changed, as they are by rearrangement, then new combinations will be stored. The same monitored efferent signal may now revive either an old or a new re-afferent trace or both. We will assume that this ambiguity is gradually eliminated by weighting in favor of the more recent combinations. If, however, conditions are such that an efferent signal becomes concurrently combined with more than one re-afferent signal, the selection from storage

[2] Taylor and Papert (16) have presented a useful analysis of the apparent movement initially induced by wearing prisms and its compensation after prolonged exposure.

must be made from among many trace combinations of roughly equal weight. Consequently, a number of revived re-afferent signals may with roughly equal likelihood be sent to the Comparator whose response will become correspondingly ambiguous. We may expect that the precision of the response of the Comparator will reflect the range of alternative combinations available in storage. The progressive degradation of coordination produced by disarrangement would be the outcome of an increasing number of stored alternatives. The same consequences could be produced by exposure to an unstable environment (external loop) which rendered the re-afferent signals fed back as a consequence of a repeated efferent signal now one value and now another. Luckily, or perhaps it could not have been otherwise, normal environments do not have too much instability of this sort of which random movement of objects is an example.

If the maintenance of stable coordination depends upon the operation of the system outlined we are forced to take account of what may be called the ecology of the sense-stimulating environment. We will have to develop schemes for quantifying those aspects of the environment that determine re-afferent stimulation. The belated recognition of the importance for coordination of exposure to these properties of the environment may in part be explained by the very ubiquity of typical conditions of exposure. Only in recent years have technological advances confronted us with the markedly atypical environments to which men are, and will be to an increasing extent, exposed for prolonged periods of time.

Up to this point we have argued entirely in terms of feedback loops and re-afferent stimulation, but there is no reason to maintain our implicit assumption that the Correlator-Comparator system can distinguish a true re-afferent signal—one that is causally related to an efferent signal by feedback through an external loop—from an afferent signal of different origin but same time of occurrence. Such signals may be of external origin (ex-afferent). But, in addition, the living nervous system is never quiescent and even though the motor consequence of an efferent signal may be precluded from arousing its characteristic response in a sense organ (by deprivation or other influence), an internally generated signal may take the place normally occupied by the characteristic re-afferent signal. This is of course the role we have assigned to internally generated noise. Under this condition the correlation between any efferent signal and the accompanying afferent signal will become extremely variable because of the variation of the noisy signal. Many combinations of a given efferent signal with concurrent afferent signals will be stored and progressive degradation of coordination may be expected.

WHY THE MODEL?

We have reviewed the evidence available and proposed a model to account for it. It seems sufficient that this conceptual nervous system simply correlates, stores, and uses the signals it receives in the manner prescribed. The proposed system neither selects nor filters the incoming signals it receives on the basis of special functional relations or orderings (other than temporal) between the efferent and afferent signals. We can hope that the model forestalls assertions that the nervous system actively seeks a special kind of order which it may store for future reference. Such statements seem to me to beg the issue by assuming an internal "intelligence" to accomplish precisely what requires explanation. On the other hand, this criticism denies neither that the immediate response of the nervous system to stimulation may be selective nor that central factors operate in perception. Rather it questions the origin and mode of operation of the selective and organizing factors.

If models of the sort proposed convincingly account for adaptive, as well as non-adaptive, psychological processes, we need

have recourse neither to a mysterious internal "intelligence" that somehow knows how to recognize and select useful sensory information nor to the equally mysterious external "intelligence" that manages to reinforce just those responses to sensory stimuli that will prove useful to the organism.

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Sensory Deprivation, Sensory Suppression and Agnosia: Notes for a Neurologic Theory[1]

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In reading the previous papers I was again struck by a similarity between two fields: experimental embryology and the study of perception. In both fields, origin and maintenance of forms are central problems; in both, study of malformations can yield important clues. Derailments of ontogeny often suggest factors that underlie normal development. Perceptual disturbances may uncover some of the physiologic bases of normal perceptual function. Eventually "illusions of the senses should tell us the truth about perception."[3]

The similarity in logical structure of the two fields extends to the types of experiments that can be carried out. In both fields, we have experiments involving isolation, recombination and defect (60, 61). By isolating an organism (or one of its parts) from its

[1] The preparation of this report has been aided by program grant M-3347 from the National Institute of Mental Health, U. S. Public Health Service.

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[3] Purkinje (9) said "... dass Sinnestäuschungen Gesichtswahrheiten sind."

usual sources of stimulation, we can attempt to gauge its relative autonomy—the degree of independence from particular inputs. By recombining parts, we can study the extent to which the normal spatial relations among parts are prerequisites for their function. By studying defects, finally, we can try to observe how destruction of a part affects the rest of the living system—how much of normal performance continues in the presence of a gap (see Table 1).

I shall attempt to show that these three types of experiments give surprisingly concordant results, when it comes to studies of perception. The results are far from establishing a neurologic theory, but might restrict one's choices and suggest additional observations. You may object that the facts derived from studies of sensory deprivation are not illuminated by citing further facts that are equally puzzling. However, all of these facts, the old and the new, seem to suggest that the central nervous system is never passive but operates upon its own inputs, emitting a continuous activity which is affected, in surprisingly similar manner, by deprivation, by recombination, and by defects. Differently put, the abnormal physiology of perception is strikingly patterned and stereotyped, and just as embryology strives to account for malformations, so any neurologic theory of perception, if we manage to have one, should take these disorders of perception into account.

ISOLATION STUDIES

Let me begin with some physiologic experiments that fit the isolation (or deprivation) paradigm. The evidence here might seem contradictory unless we think in phylogenetic terms. In the fish, it is apparently possible to cut all dorsal roots (the sensory inputs into the spinal cord) and to decerebrate the animal without abolishing motor coordination. Under certain conditions, if we accept von Holst's report (29), such a fish will show complex and coordinated swimming movements in all of its appendages. This patterning of movements seems to be independent of afferent stimulation, even that produced by the animal's own movements. It is conceivable that these 'spontaneous' motions reflect some central irritation after dorsal root section—a supersensitivity, at spinal levels, of denervated structures (19, 26, 27). Yet even if this were so, we are faced here with an unexpected independence of the motor system from peripheral sensory control.

In the amphibian, a bit higher up on the phylogenetic scale, we can cut all sensory roots except one, any one at that. As long as one sensory root is spared, a toad will walk in nearly normal fashion on a solid substrate, and swim with the usual breast stroke and frog kick when in the water (21). At this level, some afferent or re-afferent signals are needed for the maintenance of patterned locomotion, but the required signals are quite unspecific since any root will do. Cut that remaining root, and the animal is immobilized.

For the higher mammals, the picture is rather different. In the classical demonstration by Mott and Sherrington (24), it suffices to cut the sensory roots that supply one extremity, say, the arm: this extremity will then be useless to the animal, presenting all appearances of flail paralysis. Only under intense excitement does the animal move the arm at all.

These few studies suggest that there may be much greater tolerance for interruption of sensory inputs in lower forms, and a re-

TABLE 1

*Three Types of Experiment in Embryology and in the Study of Perception**

Isolation:	(A—B—C—D) or ✗—B—C—✗
Recombination:	A—B—C—D → D—B—C—A
Defect:	A—✗—C—D or A—B—✗—D

* See references at end of paper, grouped according to type of experiment.

markable autonomy of their motor systems. These systems 'play their score' (52, 60), irrespective of the peripheral effect.

RECOMBINATION EXPERIMENTS

The autonomy of central activities in lower animals (which makes them so nearly immune to radical sensory deprivation) turns into a liability when the input is changed in relation to the centers. The fly (48), fish (53), or newt (51), with eyes rotated 180°, continues indefinitely to react to the visual environment as if everything were rotated, and this maladaptive response is not corrected by experience. There are few data on forms intermediate on the phylogenetic ladder[4] (15, 40), but the recent work of Ivo Kohler (44, 45), with distorting or inverting spectacles in man, suggests an almost incredible extent of perceptual adaptation to this altered relationship between periphery and centers (36–38, 50, 54–56). I am not altogether sure (until shown by experiment) that the wearing of inverting spectacles is quite the same as inversion of the retina itself, but if it is, then we have another instance of phylogenetic shift in control, so that the relative lack of attunement to the environment in lower forms is directly related to their failure to adapt in the face of radically altered input.

However, there is a further point, already touched upon by some of my fellow symposiasts: In man, the altered input has to be systematic and orderly if there is to be adaptation (42, 43, 62). It may be useful, in this connection, to adopt Held's suggestion that we subdivide recombination studies into those involving rearrangement (orderly) and disarrangement (disorderly). This distinction draws our attention to the crucial issue: What are the conditions of adaptation and what are its limits? What kinds of new order can be tolerated by the organism? Clearly we need experiments exploring differential effects of various degrees of disorder in recombination studies. Kohler's reports abound with hints about the limits of perceptual adaptation: persistent double vision (even monocular), coexistence of upright and inverted portions of a scene, and other 'unreasonable' features of reorganized perception. All these suggest an unexpectedly piecemeal and incomplete adaptation to inverting or reversing lenses. But there are much more accessible examples for this kind of problem.

Assume for a moment simply that corresponding points are mapped onto nonhomologous parts of a dual sense organ like the two retinae. This is the situation in squint, and I urge you to look deeply into the eyes of your squinting friends; we are likely to see curious things important for a theory of perception. One of the most intriguing aspects of vision in squinted eyes is the so-called suppression of pattern vision. The condition has been called, variously, amblyopia of disuse, strabismic amblyopia, or amblyopia ex anopsia—from not seeing. All these terms reveal rather than remove our ignorance of the underlying process. It seems to me that the perceptual deficit in the amblyopic eye is quite analogous to the impaired vision of Dr. Riesen's cats. If we had a neurologic theory to account for effects of early deprivation, it might also explain the visual failure in human subjects with squint.

One or two points deserve comment: The squinted eye with amblyopia ex anopsia seems to exhibit a selective loss of pattern vision, in the absence of any significant alteration in elementary visual thresholds. Such a condition would then represent an agnosia in its purest form: a postulated dissociation of basic sensory processes (which are claimed to be preserved) from higher 'perceptual' aspects of vision which are supposedly abolished. Those of us who dislike such a distinction of sensation from perception on theoretical grounds (11, 68, 71, 73) are suspicious of any pure agnosia in

[4] See, however, the recent dissertation cited by Riesen in this symposium.

the sense here defined. For strabismic amblyopia, it has in fact been shown (59) that threshold requirements for monochromatic lights are quite normal, in spite of the profound impairment of visual 'acuity.' Subsequent studies in our laboratory and elsewhere have shown, nevertheless, that various elementary processes are quite defective in the squinted eye. Thus, Feinberg demonstrated (39) a marked reduction of flicker fusion in the central region of the retina, in the strabismic eye, and pupillary responses to light are manifestly less (41), as compared with the normal eye. What is needed are developmental studies to decide whether the squinted eye 'stays amblyopic' —as has been claimed—or gets that way, under the pressure of abnormal retinal correspondence.

To the ophthalmologist, suppression seems a useful term, because it describes what the amblyopia does; it helps the viewer to get rid of binocular double vision that might result if he did not suppress form perception in the deviated eye. Such a teleologic expression may seem to do nothing but restate the problem, but there is the observed fact of 'suppression' of pupillary responses in the squinted eye, and even of alternating suppression of pupillary responses in cases of alternating squint (41). In fact we can go one step further: Pupillary responses are suppressed in all of us during normal retinal rivalry (32).

In an experiment of this type you present a horizontal line to one eye (say the left) and a vertical line to the other. The viewer sees these lines in a stereoscope not knowing which eye gets which. During periods of binocular fusion he will report a cross; as soon as retinal rivalry sets in, he will report alternately a horizontal and a vertical line. He signals to the experimenter whether he sees one or the other, indicating in that fashion which eye is active and which eye suppressed. Throughout the test period, flashes of light are cast momentarily into one or the other eye, and the pupillary responses are recorded. It turns out that pupillary contractions are brisk and extensive in whichever eye is 'leading' at the moment, while pupillary responses of the suppressed eye are absent or diminished (32). This demonstration suggests that we might be dealing in fact with some form of active suppression during retinal rivalry. This suppression appears to act much more peripherally in the visual system than might have been suspected. It is natural that psychologists dislike the term 'suppression,' as long as it is used by ophthalmologists, but the term may point at some real physiologic process (1–3), crucially involved in some of the effects of sensory deprivation in higher species, and in some of the consequences of their restricted rearing (25, 30).

I do not wish to give the impression that this hypothetical process of suppression is always successful in minimizing perceptual disorder. People with unilateral squint tend to have double vision in the squinted eye, and this diplopia is brought out most clearly if the better eye is lost. Such patients commonly show gradual improvement in pattern vision in the remaining eye, but they may be bothered by persistent double images—a monocular diplopia. It is as if two reference systems rather than one had been built up, one organized around the eccentric point of fixation used before the loss of the better eye, the other organized around the anatomical fovea. In the best-studied case, that of Bielschowsky (34), the monocular diplopia persisted as long as the patient lived, after the loss of his better eye, a period of 16 years.

I think such a peculiar derailment of normal perception underscores what most of us have believed all along: that the spatial organization in perception is not simply given by the spatial projection of retina onto visual cortex (5). Spatial organization is a complex and active achievement (17, 20), in which appropriate reactions to movement of one's self and of the objects around us play a crucial part. How else could motor

disturbances as in strabismus produce such lasting visuo-spatial disorganization?

Drugs can be used in particularly instructive ways for disrupting visuo-motor coordination (13, 33, 46). Take the effects of drugs acting on eye muscles: If one instills homatropine into the eye, one paralyzes accommodation. The lens is set for distance, and objects lying nearby are blurred. Any deliberate effort to accommodate onto near objects leads to a striking shrinkage in perceived dimensions of whatever one is seeing —a micropsia. Conversely, one can produce a spasm of accommodation by putting physostigmine into the eye. As a result, the eye is set for objects close by, but incapable of seeing distant ones without a blur. Any attempt at focussing on distant objects leads to apparent expansion of all dimensions seen —a macropsia. These forms of micropsia and macropsia are usually cited as instances of loss of size constancy under the influence of the peripherally-acting drug. Perhaps they should better be regarded as size constancy misapplied (12, 13, 35, 47, 57). Consider what happens as long as accommodation is normal. As objects recede, two continuous and concomitant changes occur: Accommodation decreases and so does the visual angle subtended by the object. Perceptually, however, the object stays approximately the same size. It is as if a central counteractive process provided expansion of the image to compensate for its diminishing projected size. With accommodation temporarily paralyzed, the central compensatory process runs off *in vacuo*; the constancy turns into an illusion.

The same can be said about the well known disturbances in cases of acute external rectus palsy. Such a patient cannot turn his eyes to one side; when he tries, his world 'jumps.' This displacement is illusory (since his eyes have remained still), but the displacement would have been compensatory, had the eyes actually moved. The ocular movement would have transported visual objects across the retina; the illusory motion seen through the paralyzed eye goes just as far, though in the opposite direction.[5]

In fact, von Holst has postulated, as have Sperry (53) and others (6, 12, 14, 49, 58), that every self-induced movement is accompanied by a corollary central discharge from 'motor' into 'sensory' systems which prepares the latter for the changes that would ordinarily result from the movement. As long as the movements of the organism and their sensory consequences (the return signals or re-afferent impulses) are properly correlated, the corollary discharge supports perceptual constancy; alter the correlation and you have destroyed the basis for this preparatory adjustment. The corollary discharge produces an inappropriate central readiness, and the result is a perceptual illusion, rather than a constancy effect (see Figure 1).

Our hypothesis would be that it is by means of this corollary discharge from motor to sensory systems that we keep attuned to a shifting environment while actively moving in relation to it. If this hypothesis has promise, then central lesions should produce perceptual disorders not unlike those which result from interference with sensori-motor coordination (whether produced by sensory deprivation, by motor disturbances, or by spectacles distorting visual space).

DEFECT EXPERIMENTS

One of the typical changes in perception with acutely acquired lesions in the central visual pathways are micropsia and macropsia—the illusory changes in the size of objects seen. There may be systematic distortions in three-dimensional visual space, so

[5] In congenital nystagmus, the continual eye movements are not accompanied by any corresponding oscillopsia (*i.e.*, illusory to-and-fro motion of visual objects). Such an oscillopsia appears invariably in normal subjects when nystagmus is induced acutely, as by injection of sodium amytal. In patients with congenital nystagmus, sodium amytal has paradoxical effects: it temporarily stops their nystagmus (33). After the drug has worn off, these patients complain transiently of oscillopsia!

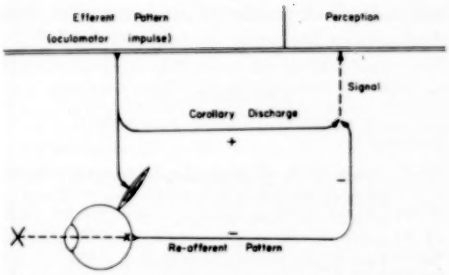

FIG. 1. Corollary Discharge (after von Holst): The diagram indicates how an active movement of a receptor surface (here, the eye) is accompanied by a preparatory central discharge from motor into sensory structures, adjusting the latter for the re-afferent stimulation which results from self-produced movement. Rearrangement of spatial relations between receptors and centers should make this central mechanism maladaptive: *e.g.*, inversion of eye or visual field should turn the negative feedback into a positive one. In higher forms, however, these disturbances can be partly overcome, as long as there is an orderly transformation from the old to the new sensorimotor pattern.

that objects seen in an affected (homonymous) half field or quadrant are consistently mislocalized (74, 75). Most frequently, objects are seen as too far ('teleopsia'), and conjointly as too small ('micropsia'), compared with their appearance in less affected regions of the field. The disturbance can be demonstrated with variations of the three-needle experiment (63); on the stereoscope there are difficulties with binocular fusion, and distortions akin to those produced in normal subjects wearing aniseikonic lenses (31). The disorder is found in the absence of any demonstrable impairment of cerebellar function, postural sensations, or (peripheral) vestibular reactivity. The syndrome may persist for months after acute occipital lobe lesions, but is rarely found several years after injury. However, at these later stages the distortions may appear transiently during paroxysmal changes in visual function ('visual fits'), in which the EEG shows focal alterations (74). Curiously, these paroxysmal visuo-spatial disorders are significantly more frequent with lesions of the posterior lobe substance in the right (rather than left) hemisphere (74).

Still more revealing are the various disorders of motion perception in these cases. During motion of objects, or movements of the eyes, images often become multiple (75). There may be recurrent or persistent monocular diplopia, or polyopia (multiple vision with one eye, either eye). In extreme cases there appears a peculiar form of visual perseveration, so that the seen motion of objects is dissected into a series of motionless frames. One of our patients complained, for instance, when a motorcycle passed him on the defective side of his visual field, that he saw "a string of motorcycles standing still." With the curious predilection in neurology for compounding Greek names, the disorder has been called 'palinopia' (65, 74). There is an allusion to a similar case in Wertheimer's famous monograph on motion perception (16), the founding document of Gestalt psychology.

Analogous symptoms appear in the somatosensory modalities with acute parietal lesions or fits arising from these structures (7, 8, 66, 69, 70, 72). Patients complain of persistent or recurrent distortions in the felt size of limbs, of illusory motion of body parts, or of multiplication of limbs, with 'parasitic' hands or feet; a rather disconcerting experience.[6] To speculate on the physiologic basis of these troubles may seem premature in the present state of our knowledge, or rather ignorance, of physiologic bases of perception, but we should bear in mind how surprisingly stereotyped these disturbances are (4). Quite similar disorders can be transiently produced by certain centrally acting drugs, such as mescaline (64, 67) or LSD-25. In fact, when I first heard Professor Hebb's account of the McGill

[6] Quite a different picture is obtained in cases of peripheral loss of a sensory surface, as after partial amputation of a limb (22, 28). Here, marked improvement of sensory acuity occurs on the stump, making this part superior on numerous sensory tests to the homologous portion of the intact limb.

deprivation studies (18, 23), I had come late and had not heard the beginning of his account. When he paused, I asked "how many gamma (millionths of a gram) did your subjects take?" I had thought erroneously that he was relating an LSD session.

The manifest uniformities in effect of such chemical lesions and those following the more typical defect, or recombination, or isolation studies, require a common mechanism for their interpretation. I am inclined, with Dr. Freedman, to see this mechanism in the central activities by which incoming sensory signals are adjusted to emerging efferent activity. The decay in perceptual constancies during deprivation, I should think, is due to the free-wheeling of some active process, rather than to mere effects of some internal 'biological noise.' All this is vague and does in no way constitute a neurologic theory of sensory deprivation. Still these reflections help me in putting together a number of phenomena that are usually considered in isolation, and the way in which the facts seem to fit together might raise one's hope for a theory.

There is also the promise of experimental tests. I am thinking especially of the fiendish variation, suggested by John Smythies (10), of a radical Kaspar Hauser experiment: fit a young child with a television set, so that he sees a continuous program unrelated to his own locomotion. This is, of course, a fate worse than death (considering the kind of available programs), but the question would be: Could such a child acquire normal pattern perception? If there is merit in the notion we have collectively developed in this symposium, then the answer should be no. The child should have profound agnosia. But would he?

This is all. In looking back on it, I am reminded of what Professor Whitehead said after Bertrand Russell had given a talk on quantum mechanics. Whitehead got up, thanked him kindly, and added (with the famous sparkle in his eyes) how grateful he was to the speaker, and "not the least because he had managed to leave the vast darkness of his topic unobscured."

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Discussion

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These will necessarily be random comments. We are still in search of that theory, I think, but the present symposium speakers have arrived at some useful theoretical ideas, so we are making progress. There are times when I wish I had never heard of perceptual deprivation or isolation or whatnot. I had something to do with it (but please remember that it is Heron, Bexton, Scott and Doane who have the main credit for whatever success we had at McGill) and consequently seem expected at times to explain the results. I am incapable of so doing. However, I am supposed to be a discussant, so let me discuss.

It seems important to emphasize Dr. Freedman's separation of "sensory" from "cognitive." It is not certain, however, that the visual hallucinations are a primarily

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cognitive disturbance, though the content is certainly cognitive enough. My point is that they are specific to visual deprivation rather than general deprivation, as far as I can discover. There is one aspect of the results of deprivation that particularly shows the value of distinguishing sensory from central. This is the occurrence of apparent improvement in sensory function at the same time that central or cognitive functions are steadily deteriorating. Doane found his subjects to have better visual acuity at a nearly significant level ($p = .10$), and Mahatoo did obtain significant improvement in the two-point limen, at the same time that tactual pattern-perception was significantly worse (1). Need I add that these results confuse me?

The results presented by Dr. Riesen are obviously significant—if also puzzling in some ways—and I certainly agree with his approach. But I am not sure in just what way we should maintain that separation of motor from sensory and central events. Perhaps my point is only that, having separated them conceptually, we should then emphasize their collaboration in the control of behavior; sometimes, at least, "perception" is regarded as a wholly distinct process from motor activity, and this I think is not so.

Trying really to understand what went on in visual perception, as realistically as I could in physiological and anatomical terms (but still trying to be a psychologist, not a physiologist), I could find no way of avoiding the conclusion that the motor outflow occurring at the same time as the input constituted an essential part of the process. Then subsequently I tried applying the same reasoning to somesthetic perception, particularly kinesthetic perception (a badly neglected topic in psychology). I came to the surprising conclusion—surprising to me—that what I was talking about was skilled movement: in other words, motor learning. So what I called perceptual learning or perceptual development, in the field of vision, was visual-motor learning; and what I called motor learning was somesthetic-motor learning, not a different thing at all but quite parallel to the visual phenomena.

As to the re-afferentation question, however, it seems that there are limits to the proposition that the subject has to be active in order to benefit from the effects of stimulation. Hymovitch (4) and then Forgays and Forgays (3) reared rats in very considerable motor restriction. The rat grew up in a small cage, of diameter equal to the length of his head and body; he could turn around but not locomote, and could not rear up on his hind legs. The cage was placed daily in a new position in the experimental room, thus giving a wide visual experience. In these circumstances the rats were nearly as capable of a maze performance as the animal reared in a "free environment," and better than rats reared in small cages without the additional visual experience. Here then are animals that lacked, almost entirely, the experience of going from here to there, who none the less proceeded to do quite well at a task which now required them to do so. They were not, of course, without experience of the effects of motor activity. The effects of head and eye movements, and approach movements on a small scale, *were* experienced; but it was surprising to me that no more than this was needed as a basis for maze learning.

Dr. Held's discussion of the rapidity with which changes in behavior are induced, with retinal inversions and so on, reminded me of a conclusion I had come to on the basis of a quite different kind of experimentation. Eccles and his colleagues (2) showed that synaptic function in the cord is impaired by deafferentation, but can be very rapidly improved by post-tetanic potentiation (activating the synapse at the rate of 400 impulses per sec. for 15 sec.). Eccles has treated this as the paradigm for forgetting and relearning. But we know that simple disuse of a conditioned reflex does not produce forgetting in a matter of weeks, the time

needed for impairment in this experiment. Also, there is reason to believe that if a child has had vision for four years or so and then becomes blind he never loses the effects of the visual experience, despite complete disuse. So it seems to me that we may be dealing with two kinds of process here, both of which perhaps can be called learning, but one of which is more fundamental and long-lasting, the other being a more superficial and transient effect superimposed on it. The rapid acquisition or rapid loss amounts to a modification of some learning which is more stable, occurring during the developmental period.

This is a point, probably, at which the conception of "learning" is too crude, too gross, for the purposes of our analysis. At the present stage of knowledge we may have always to specify what kind of learning we are referring to in any particular case. Dr. Teuber, could you supply us with some Greek names for the two kinds I have mentioned—assuming they exist?

On the question of visual "noise" (this refers to both Dr. Teuber's and Dr. Held's papers) it is important to observe that as far as present knowledge goes the rate of random activity in the CNS in darkness is as great or greater than in conditions of visual stimulation. Should this not mean that putting the subject in complete darkness will cause as much disturbance as putting him in unpatterned light? That is, if noise is the explanation. But it does not; the effects are greater with unpatterned stimulation. Dr. Heron has suggested to me that the explanation may be that we are, after all, thoroughly used to periods of darkness but are not used to unpatterned visual stimulation. The result may be some sort of learning process that has organized the "noise" of darkness into something less noisy, but has not done so for unpatterned light.

It is necessary also to keep in mind, in connection with Dr. Teuber's remarks about the fish, the toad and the paralyzed monkey, that the noise level in a higher animal ought to be much greater than in a lower one, with smaller CNS. The higher animal is equipped with the neural tissue to solve many more problems than the one specific one he is now dealing with, and this superfluous tissue may not be willing to be quiet when it is not used. The evidence is pretty clear that when you give an animal of a lower species the kind of problem to solve or the learning to do for which he is specially equipped, the learning which goes on is as quick as—or quicker than—that of chimpanzee or man. I am thinking of Baerends' experiment (5) on the solitary wasp, where there is clearly marked one-trial learning, for example; or in general, the abundant evidence to show that a lower species may learn as quickly as a higher one. There are two points, then: first, the probability on anatomical grounds that the noise level is higher in the more complex brain; and secondly, the evidence from the toad and the fish. All this can be seen as support for the noise idea in the present context: that is, the fish has a low noise level, the toad somewhat higher, and man of course a much higher one.

With regard to Sperry's work, there is some interest in the parallel between the retinal-inversion and his experiments with motor functions—it is not only his eye-rotation experiments that are relevant. He interchanged the innervations of flexors and extensors in the rat's hind leg; for the rest of the animal's life he flexed when he needed to extend, and vice versa. Next Sperry did a similar experiment with the monkey's forearm; at first he got the same result, but soon the monkey made the necessary corrections, so to speak, and flexed when he meant to flex—except, if I remember rightly, when excited: then the errors returned. What Sperry showed is that the larger brain of the monkey could compensate for the "incorrect" connections of the spinal cord, by a learning process; but the brain of the rat was not up to this. The parallel is very striking with the retinal inversion experi-

ments. Retinal inversion in the amphibian, by surgical rotation of the eyeball, is permanently disorganizing; retinal inversion in man, by the use of optical devices, is grossly disorganizing at first but then is compensated for by a superimposed learning.

On the lazy-eye business that Dr. Teuber spoke of, it seems incredible that there has been no psychological research. There is a plentiful supply of subjects (you have to ask for them, they generally don't have a squint to give them away). I met a student once who should have been studied—or, since he was a graduate student in psychology, he should have studied himself. He used one eye only, but it seemed not to matter, very much, which eye it was. He had found, in his attempts to coordinate the two eyes, that covering up the one currently in use would result in a week or so in bringing the other eye up to the same level of function. But now the eye that he had not been using was unusable. The research possibilities in such a situation (with respect to the mechanisms of perceptual learning, or learning in general), and in the observations of the kind Dr. Teuber discussed, seem to me very great. The ophthalmologists are all familiar with such problems, but psychologists seem not to have heard of them.

Let me conclude by saying that I am still unable to explain the effects of perceptual isolation, but the speakers we have heard have given us some significant further material and made steps toward understanding these phenomena.

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SEXUAL, AGGRESSIVE AND ACQUISITIVE DEVIATES

A Preliminary Note[1]

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Psychoanalytic experience has shown that characterological needs will be expressed by remarkably consistent psychic mechanisms and behavior, either overtly or through defense maneuvers. If the external milieu be altered, a transient variation in the behavior, ideation, affect, or symptoms may result, but unless some far-reaching intra-psychic change occurs, the patterns will reappear. A repetition compulsion, operative in neurotics, will imperiously assert itself, adjusting to, rather than deterred by, the environment. It is a reasonable postulate that if this is true of neurotic behavior (and, indeed, of normal and psychotic behavior), it occurs also in what I have called polistaraxic or socially provocative antisocial behavior. It was our hypothesis that sex, aggressive, and acquisitive offenders had significant, internally consistent characteristics which, properly studied, would lead to increased insights into their psychogenesis and dynamisms and thus augment our prognostic facility and enhance our therapeutic potential.

In this report, I shall briefly describe a few of our approaches. Our subjects were selected for us by a ritualized sequence of socio-legal steps. This limits our generalizations from them without cautious qualification on the one hand and bold conceptualization on the other. This is not entirely a disadvantage. In physics we may be made aware of the distribution of externally impinging forces by a knowledge of the internal disposition of molecules of gas; just so we are able to speculate concerning the selective responsivity of the community by the distribution of offenders incarcerated in its prisons. We hope to obtain information concerning the enormous resonance which exists in the general population for these kinds of behavior. The ubiquity of secret criminalistic impulses and fantasies which are aggressive, sexually polymorphous and acquisitive, revealed in the psychiatrist's consulting room, and a variety of sociological evidence, all demonstrate that the criminal is by no means an isolated phenomenon in our culture. He is a kind of crystallized expression of an aspect of the normal as well as the disturbed personality, which is the general concern of psychiatry no less than of other behavioral and social sciences.

Our analysis of fairly widely ranging data seems to justify not only the general hypothesis that antisocial behavior is symptomatic and meaningful, just as is intra-psychic behavior, but that specific forms of polistaraxic behavior are pathognomonic of definable and peculiar patterns of development and character. Our criteria describe discrete groups relatively homogeneous within themselves and heterogeneous when compared to each other, and we are analyzing variants within each of these groups. We are sketching out differential community operations eliciting and responding to these types of behavior. Perhaps most important, we are developing clusters of factors which are statistically significant at the level of overt

[1] The paper was written with the assistance of the Foundations Fund for Research in Psychiatry. An earlier version was presented at the one hundred and fourteenth annual meeting of the American Psychiatric Association, 1958, San Francisco, California. In the five-year period of the pre-study and study a number of professional colleagues, members of the staff of the Yale Study Unit in Psychiatry and Law, contributed to this project. Of particular relevance to the present report was the valued participation of Dr. Hugh Storrow, psychiatrist, Dr. Jacques Kaswan, psychologist, and Dr. Raymond Forer, sociologist.

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behavior, and are conceptualizing patterns of factors at clinical and psychiatrically symptomatic levels.

This, then, is an introductory report of a study concerned with aggressive, sexual and acquisitive deviates which has been conducted, under psychiatric auspices, by an interdisciplinary team of psychiatrists, psychologists and sociologists. Subjects were prisoners in a northeastern state who had been institutionalized for socially disruptive behavior. We define sexuality as activity which is substantially concerned with erotic or genital stimulation. Aggressivity has to do with forceful and harmful action directed at another person. Acquisivity is a generic term employed by us to subsume illegal acquisition of property without aggression. Obviously no aggressive, sexual or acquisitive individual exists whose behavior may be comprehended in such absolute terms. In psychiatry as in other branches of medicine, however, clinical entities may be delineated by selecting the principal sign or symptom as the nucleus of a syndrome for purposes of description, nosology and communication. Its meaning then hinges upon whether it can be consistently integrated into a pattern for diagnostic, prognostic and therapeutic uses.

Of these three categories, sexual and aggressive deviates have most often been the concern of psychiatrists for diagnosis or treatment. Certainly they are major unresolved clinical as well as social challenges. They take on further scientific significance because of the crucial role which sexuality and aggressivity play in the psychoanalytic theory of personality development. The situation, however, is otherwise with our third category. For psychiatrists do not treat, and analysts do not analyze, run-of-the-mill thieves. Occasionally a compulsive embezzler or shoplifter comes to therapy and generalizations are made from them. But these cases are not representative of the mass of illegal, acquisitive offenses which, in fact, form the quantitative bulk of socially and legally disruptive behavior in our acquisitive society.

Behavior which provokes social outrage, anxiety and punitive sanctions by the community has, thus far, been the subject of few objective, systematic psychiatric investigations. We have named such activity polistaraxic behavior, a new but, we think, a necessary and useful term containing within it the concept of conduct which provokes the social group in which it occurs to negative-sanctioning counteraction. The phrase reflects the model of hypotheses with which this investigation was conceptually undertaken, using sociological as well as psychological and psychiatric techniques to study socially arousing behavior within the context of the group response which it evokes. Symptoms which reflect internal conflict have been called autoplastic, and those in which conflict is directed outward, alloplastic. The term alloplastic behavior, however, is only partial, since while it refers to "acting-out," it indicates the direction of motility but not the object; human behavior always involves other persons whether immediately present or fantasied, whether by anticipation or by reminiscence.

METHOD

This investigation followed a two-year period of pre-study involving some 1500 inmate subjects, the results of which largely determined our present research design and postulates. Our method consisted basically of structured and therapeutic psychiatric interviews, participant observation in group psychotherapy sessions, sociological interviews, psychological testing, some field surveys of court proceedings, and study of case records—all conceived and analyzed within a psychoanalytic model of personality and culture. The psychological tests included the Rorschach, Blacky, Picture-Frustration Study, Bender-Gestalt technique, a specially adapted Attitude Scale, and the Army Beta, a non-verbal group intelligence test. Because of our belief

in the relativity of scientific perspective, we also tried to gain some insight into operations used and values reflected by ourselves as investigators and psychiatrists.

One hundred and fifty men, fifty in each category, were studied. Each subject was selected from a population of 800 men by a random stratified sampling technique. For each of the three classifications, we first computed the distribution on ten variables determined in the course of our pre-study. These were: age, race, marital status, religion, education, IQ, psychological disturbance rating, minimum sentence, time in prison, and type of previous offenses. We then randomly selected fifty subjects from each of the groups so that their distribution in each of the ten variables matched as closely as was possible the distribution of the offense group from which the sample was drawn. Each of our sample groups was, therefore, representative of the total population of sexual, aggressive, or acquisitive offenders in the institution at that time.

RESULTS

In this preliminary communication I shall present some observations concerning the psychosexual behavior of subjects in all three groups. We have proved to a high degree of statistical probability the existence of these groups on a number of clinically identifying and significant variables. This report, however, is a selective comparison of certain related factors and clinical impressions derived from them, presented out of the context of the complex of interlocking psychosocial phenomena.

Our subjects came from the lowest socio-economic classes, but there were nonetheless socio-ethnic differences between them; as, for example, the preponderance of geographically mobile Negroes in the aggressive group, contrasted with the white, indigenous, rural New Englander in the sexual group. The consistently low socio-economic status and preponderant minority group origin indicates socially imposed vulnerability, and certain characteristics of their ease of detection points toward self-imposed amenability to punishment. In addition to demographic differences, intrafamilial relationships with father, mother, and siblings showed characteristic and possibly clinically significant variations, as did the kind and quality of peer group and community interaction.

The kind and quality of these findings were reflected to a remarkable degree in the psychosexual lives of our subjects. We might summarize these data by describing our sexual group as, in fact, deviate, our aggressive group as ambivalent, and our acquisitive group as resilient. We will divide our psychosexual data chronologically between experiences before the age of 16, and those after 16. That is, we will distinguish psychosexual phenomena occurring during the pre-oedipal, oedipal and latency periods and the first thrust of puberty, from those occurring in early adult and adult life when genital maturity, in the ordinary course of events, should have been well-established.

The most statistically significant and clinically striking contrasts and similarities in our comparative analysis of psychosexuality occurred between the sexual and acquisitive subjects; this was also the case in other parameters of behavior. The aggressive subjects were in most variables intermediate between these extremes, but were far from the stereotype of virile masculinity that one might expect of the self-assertive attacker of other persons.

The overwhelming majority of men who later became sex deviates were deeply inhibited during early childhood and especially during the crucial pubertal stage from all forms of manifest sexual outlet. They had had a significantly lower level of overt sexual behavior of all kinds than any other group ($p < .05$). As might be expected, they had far less heterosexual play and coitus with girls than the others. Their childhood practice of homosexuality was surprisingly low; in fact, somewhat lower than those who later

became thieves. Their incidence of masturbation was also far less than these acquisitive subjects. They tended to be the oldest at the time they achieved their first ejaculation, which in turn was likely to be attained by masturbation rather than through any physical or psychical relationship with another person. Those sexual deviates who did have homosexual activities before 16 were likely to have done so with boys younger or their own age, while those aggressives and acquisitives who engaged in homosexuality did so generally with older men.

There was a curious and possibly significant dichotomy in the early history of the sexual deviates. As a group, they were inadequate or inferior in the overall vitality of overt sexuality, but in a small but definite minority (about 10 per cent) just the opposite was true. These had started masturbating and, indeed, had attempted heteroxesual coitus before the age of 11; thus they had somehow been prematurely stimulated during the latency period. This intrusion into what is commonly experienced as years of sexual quiescence may help us to understand the predisposition and the direction which their perversion assumed in later years. None of our acquisitives, and only a single aggressive subject, reported such early coitus. Those deviates who started early activities had a greater frequency of such activity than the comparative groups. At the level of clinical observation, those who had been stimulated to premature coitus were sexually aggressive toward women; those who had indulged in early autoerotic behavior were more likely to become homosexual pedophiles.

The aggressive and acquisitive subjects experienced a sharp and dramatic upsurge of phallic sexual activity which was chronologically coincident with the onset of the pubertal libidinal thrust. Most of our acquisitive, and almost half of our aggressive subjects, had their first coital experience between their eleventh and sixteenth birthdays. For the sexual deviate, heterosexual intercourse was postponed to far later in adolescence or to adulthood. Again, the majority of the acquisitive group began masturbating during their earliest adolescent surge between eleven and fourteen; among the sexual group, however, only about one in five responded in either of these ways. For them both private masturbatory as well as interpersonal coital behavior occurred late in adolescence or early or middle adulthood. Nor was homosexuality used as an alternative outlet. Only about 10 per cent had such relationships with any frequency before the age of 16, and an additional four per cent had very infrequent homosexual interaction.

In adult life, however, the overall picture is that of bi- or ambisexuality rather than uniformity in selection of sexual object.[3] The sexual deviates were just as apt to be married as the others. Moreoever, compared to the aggressives, they had intercourse with their wives more frequently, and were more likely to initiate it.

If there is any single quality that may be said to characterize our aggressive offenders, it is their ambivalent approach to their sexual life. In general, the heterosexual profile of this group is intermediate between the sexual and acquisitive groups. They resemble the acquisitives in some ways, but there are areas where their sexual inhibition and timidity approaches and even exceeds that of the sexual deviates. They tended to be more ambivalent in their feelings about heterosexual relations, were less likely to take sole initiative in marital relations, and engaged in less intercourse with their wives than sexual and acquisitive offenders. They were the most inhibited in fantasying during masturbation, and fewer expressed gratification or relief, while more gave evidence of ambivalence after masturbation than any

[3] This impression of a melding of sexual drives and objects in adult life (within the pattern of their psychosexual diversity) is enhanced by our observation that while in prison, in the absence of appropriate objects, approximately the same percentage of all groups reported homoerotic activity.

other group. Feelings concerning sexual daydreams were most likely to be "blocked" and not articulated. Of those who responded, most reported their feelings as unpleasant.

Acquisitive offenders showed, in general, a significantly higher level of overt sexual behavior of all kinds than any other group. They had a higher incidence and frequency of heterosexual activity; more of them had had homosexual relationships than the aggressive subjects, and about the same percentage as the sexual groups. Almost all started masturbation in their early teens. They tended to be younger at the time of their first ejaculation than any of the others, and were far more likely to have participated in coital activity the earliest. Possibly because of their other outlets, the frequency of masturbation before puberty was the lowest of all, and they were most likely to have ceased by the age of 17. They were most likely to have engaged in coital intercourse both in and out of marriage, to have initiated sexual relationships with their wives and, for their non-marital partners, were more likely to have used either prostitutes or women with whom they had had casual and transient relationships. Fewer, indeed, were married than in the other groups. Sexually they were the most active, least rigid of the groups and (superficially at least) the most accepting of their own sexual proclivities. They accepted their homosexual experience without noticeable upset as compared to the aggressives. They were, however, activists and "realists," apparently able to tolerate sexual activity far more easily than sexual imagery or fantasy. Most of them rejected sexual daydreams as being unpleasant.

DISCUSSION

Several impressions emerge from these findings. There is the early onset of peculiar and persisting patterns of behavior. There is a remarkable unity of seemingly disparate parameters of behavior. For example, from his earliest history, the acquisitive offender is sexually a free-acting, poly-focal being. Although he is most active, his tend to be the most transitory sexual relationships. He is most diverse in his objects, with fewest obvious indications of negative reactions to his sexual experiences. He is most directly responsive to physiological alterations of motivation and adjusts easily to externally imposed variability of available objects. These psychosexual characteristics take on significance when correlated with his pattern of objectless, repetitive illegal acquisition of material goods. There is a parallelism between his sexual relationships and the sociopsychological qualities of the acquisitive acts themselves. Here too he is (we might almost say compulsively and promiscuously) repetitive and reveals least concern about them. The interchangeability and relative personal insignificance of his sexual objects as compared to the persistence of his sexual aims is matched by the human objectlessness of his persistently acquisitive aims.

To cite another example of apparent identity between private sexual mores and the type of antisocial act, the aggressive is the most heterosexual of all, if we take absence of homosexuality as our criterion. However, we have evidence to justify the assumption that he is goal-inhibited rather than overwhelmingly heterosexual. His marital life is more passive and less active than the other groups. It is striking also that the acquisitives reported more fighting and less control of anger than the aggressive subjects. (Since this is self-judgment, it may reveal differences in evaluation of what constitutes control.) In fact, many of these aggressives were clinically passive people who had lashed out during a passive-aggressive crisis in their lives. The aggressive behavior might therefore be a reaction to the active-passive conflict, which seems to be reflected also in their sexual history.

Thus, as we have seen, certain aspects of sexual behavior may be revealing clues to an understanding of seemingly disparate aggressive and acquisitive activity. We have found other comparably significant parallels

in the developmental and adult patterns not only in sexual, but in the aggressive and acquisitive spheres as well. That is, we have been able to relate certain repetitive elements in each of these phases of personality to their characteristic modes of antisocial behavior.

Our data similarly permit observation and correlations concerning the social meaning of this behavior. Our preliminary findings would seem to indicate that the acquisitive offender is much more typically a subcultural phenomenon, in that there is variance in the values of his entire group and those of the dominant sanctioning community. Our sex and aggressive criminals reflected a more individual psychopathological response. In terms of social conformity and adjustment, the acquisitive subjects manifested a kind of group anomie (or normlessness) in that their subculture seemed anomic *vis à vis* the dominant culture. The sex and aggressive offender seemed to represent a personal anomie, out of the misery and disturbance of their own lives. This last observation probably explains the pseudo-debate which goes on between the sociologist and psychiatrist

VERBAL AND CONDITIONED AVOIDANCE LEARNING DURING ABNORMAL EEG DISCHARGE[1]

VICTOR MILSTEIN, PH.D.[2] AND JANICE R. STEVENS, M.D.[2]

INTRODUCTION

The purpose of this study is to determine the influence of diffuse synchronous spike-wave and slow-wave activity in the EEG on the ability of patients with convulsive disorders to learn new material. A number of reports in the literature deal with the effects of epileptic discharge on learning ability in both human and animal subjects (3, 6, 8, 9, 12, 16); these studies, however, do not clearly delineate the effect of an ongoing epileptic discharge upon the learning process.

In a previous study of four patients with convulsive disorder, conditioning of a light-elicited spike-wave discharge to follow a paired tone signal could not be demonstrated (17). Walter (18) reported difficulty in conditioning the subharmonic driving response to flickered light elicited from certain epileptics and suggested that these abnormal responses may be due to a disturbance of the learning mechanism of the brain. Impairment of the integrative functions of a cerebral centrencephalic system as proposed by Penfield (14) would provide a possible explanation for a learning disturbance coincident with diffuse EEG epileptiform discharge.

In previous studies concerned with conditioned epileptiform discharges (2, 5, 7, 10, 13) the paroxysmal activity occurred as the last step in the conditioning sequence. In the present study, the abnormal discharge either encompasses the entire interval during which the material to be learned is presented, or else is initiated by the conditioned stimulus (CS) and so occurs as the first step in the learning process. The hypothesis being tested is that the presence of diffuse spike-wave or slow-wave EEG activity will interfere with learning ability.

SUBJECTS

Nine subjects over 15 years of age with convulsive disorders who had previously manifested a diffuse, abnormal EEG response to intermittent photic activation were selected from the patient roster of the Clinical and Research EEG Laboratories of the University of Oregon Medical School. These individuals demonstrated an abnormal response to flickered light stimulation at least seventy-five per cent of the time. The abnormal potentials elicited by light were not commonly present in the non-activated EEG.

PROCEDURE

Preliminary: The subject lay supine on a bed and needle electrodes were attached to the scalp. Bipolar recordings were made on four channels of an eight-channel Grass Model III-D Electroencephalograph between mid-frontal and occipital leads, anterior and posterior temporal electrodes, bilaterally. The remaining four channels were used for signals and measurements of muscle response (EMG) and skin resistance change (GSR). The light of a Grass Photo Stimulator, Model PS-1, was placed twelve inches from the subject's face so as to illuminate both retinae equally. Previous photic activations with each subject had indicated a range of frequencies which evoked abnormal EEG activity. A frequency

[1] Based on a dissertation submitted to the Graduate School of the University of Oregon in partial fulfillment of the requirements for the degree of Doctor of Philosophy. Presented in brief at the American Psychological Association meetings, Cincinnati, September, 1959. Supported in part by U. S. Public Health Service Grant No. B-1140 (C2).

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of flickered light within this range was presented and termed the "p light"—flickered light stimulation which evoked abnormal activity in the EEG. A frequency outside of this range was also presented which did not result in an abnormal response and was termed "np light"—flickered light stimulation which did not evoke abnormal EEG potentials. Stimulation with "np light" typically evoked a driving or blocking response in the EEG.

Verbal learning: Three lists of nine two-syllable paired adjectives were constructed from those presented by Hilgard (4). The nine words in List 1 had a mean association value of 2.75; those in List 2 of 1.97; those in List 3 of 0.66; hence the lists were of increasing difficulty. The nine words in each of the three lists were randomly assigned to three groups, A, B, or C. The three words in Group A were read to the subject while he was being stimulated with "p light" which evoked pathological potentials. The words in Group B were read while the subject was exposed to "np light." The Group C paired adjectives were not accompanied by photic stimulation. To eliminate possible position effects on learning, each list was randomized three times, resulting in 27-word lists. A restriction placed on the randomization was that words in Group A were not to occur in succession; *i.e.*, flickered light which elicited abnormal activity was not to be presented consecutively because of the possibility of precipitating a frank seizure in those patients responding with spike-wave activity. The three lists and the random order of test words are presented in Figure 1. An interval of 10 seconds elapsed between the reading of each pair of adjectives. For the words in Groups A and B, the appropriate frequencies of flickered light ("p light" and "np light") were turned on approximately one-half second prior to reading each pair and terminated immediately after it was read.

The following instructions were read to each subject before List 1 was presented:

"I'm going to read you a list of words two-at-a-time. I want you to listen carefully because after I'm through, I'll expect you to remember the words that went together. For example, if I read UP-DOWN, IN-OUT, then when I would say UP, you would say . . . .; and when I would say IN, you would say. . . . Sometimes, the light will flash and sometimes it won't. I don't want you to pay any attention to it; just concentrate on the words and see how well you can do. Don't worry if you don't hear or understand some of the words, because I will be reading them more than once. Any questions?"

Recording of EEG and other variables was then commenced. The twenty-seven paired adjectives in List 1 were read, each accompanied by the indicated flicker or by no light. The position of each pair of adjectives was marked on the tracing. After the list was completed, the EEG recording was stopped, the test words presented and the subject's responses scored. The second and third lists followed.

Conditioned avoidance learning: Following the verbal portion of the study, earphones which transmitted the noise from a simple door-buzzer were placed in the subject's ears and the intensity of the noise adjusted so that it was unpleasant but not painful. A key with which the subject could turn off the noise was placed under the subject's left hand and manipulated several times until he became familiar with its operation. Small needle electrodes were inserted into the flexor muscle of the left forearm to record muscle potentials. GSR measurements were taken from the palm and back of the right hand and were continuously recorded as resistance changes with a DC amplifier on the same tracing as the EEG, EMG, and key-press signal.

Two frequencies of flickered light within the range yielding abnormal activity were presented separately and the subject was asked to press the key after the faster of the two lights to determine whether he could

No.	*Gp.*	*List 1*	*List 2*	*List 3*
1	A	perfect-faultless	feeble-infirm	noxious-toxic
2	B	noonday-midday	dextrous-adroit	acute-astute
3	C	exact-precise	stuffy-stifling	limpid-lucid
4	A	boastful-bragging	recent-modern	zigzag-forked
5	C	fatal-deadly	costly-precious	natal-nascent
6	B	ready-prepared	witless-stupid	waggish-jocose
7	C	foremost-leading	handsome-comely	obese-bulbous
8	A	total-entire	equal-alike	yeasty-spumy
9	B	empty-vacant	bloated-swollen	dulcet-tuneful
10	C	exact-precise	stuffy-stifling	limpid-lucid
11	A	perfect-faultless	feeble-infirm	noxious-toxic
12	C	foremost-leading	handsome-comely	obese-bulbous
13	B	noonday-midday	dextrous-adroit	acute-astute
14	B	ready-prepared	witless-stupid	waggish-jocose
15	A	total-entire	equal-alike	yeasty-spumy
16	C	fatal-deadly	costly-precious	natal-nascent
17	B	empty-vacant	bloated-swollen	dulcet-tuneful
18	A	boastful-bragging	recent-modern	zigzag-forked
19	C	fatal-deadly	costly-precious	natal-nascent
20	B	empty-vacant	bloated-swollen	dulcet-tuneful
21	A	perfect-faultless	feeble-infirm	noxious-toxic
22	C	foremost-leading	handsome-comely	obese-bulbous
23	B	noonday-midday	dextrous-adroit	acute-astute
24	A	boastful-bragging	recent-modern	zigzag-forked
25	C	exact-precise	stuffy-stifling	limpid-lucid
26	A	total-entire	equal-alike	yeasty-spumy
27	B	ready-prepared	witless-stupid	waggish-jocose
1	B	empty-	bloated-	dulcet-
2	B	noonday-	dextrous-	acute-
3	C	fatal-	costly-	natal-
4	A	boastful-	recent-	zigzag-
5	C	foremost-	handsome-	obese-
6	A	perfect-	feeble-	noxious
7	B	ready-	witless-	waggish-
8	C	exact-	stuffy-	limpid-
9	A	total-	equal-	yeasty-

FIG. 1. Lists of paired adjectives and order of test words.

discriminate between them in this fashion. Two frequencies which he could discriminate were treated as the "CSp" and "DSp"; the conditioned and discriminative stimuli which evoked abnormal activity on the EEG. A similar discrimination was made in the range of flickered light which did not evoke abnormal discharges, and the two frequencies selected were termed the "CSnp" and "DSnp"; the conditioned and discriminative stimuli which did not evoke abnormal potentials. One subject responded to only a single frequency of flicker with spike-wave discharge; consequently, discrimination procedures could not be employed with this patient.

The following instructions were then read to each subject:

"*At certain times, that buzzer you heard before will come on. There are two things that you have to do. First, you must figure out when the buzzer is supposed to come on; then you must figure out how to stop it. You should try to keep the buzzer from sounding at all. If what you try first doesn't work, then try something different. It's important that you do what has to be done to keep the buzzer from sounding only when you expect it, and not all the time. Any questions?*"

If, during the first conditioning procedure, the subject pressed the key only after the buzzer sounded, he was told to:

"*Try to keep the buzzer from sounding at all.*"

This was said to any patient only once.

One-half of the subjects started the conditioned avoidance learning with the CSp;

the other half started with the CSnp. Conditioning trials were presented until the subject reached the criterion of five successive complete avoidances of the buzzer—the unconditioned stimulus (US). The correct response was depression of the response key during the CS and throughout the US period. The duration of the CS (flicker) was two seconds unless it threatened to precipitate a convulsion, in which case it was reduced to one or one and one-half seconds. As the flicker terminated, the buzzer started and lasted for two seconds. Trials were presented at irregular intervals every 30 to 90 seconds.

Following achievement of the criterion of conditioning, ten discrimination trials were presented randomly interspersed with five additional conditioning trials. The correct response to the DS was "non-key press" during the two-second interval of the buzzer. After the discrimination trials had been presented, extinction trials were given until the subject did not press the key during five successive trials. The correct response during extinction was again "non-key press." If, during any of the three conditioning procedures, the subject failed to make the correct response or made an incorrect response, the buzzer sounded. Further, if the duration of the correct response was not sufficiently long to cover the US interval, the buzzer sounded.

RESULTS

Reliability of EEG response: The EEG responses to the various frequencies of flickered light stimulation were broadly considered as either normal or abnormal. Normal responses consisted of blocking or driving. Blocking was considered to have occurred to the flickered light stimulus if there was any increase in the frequency of the waves coupled with a decrease in their amplitude. Some subjects, at times, manifested a "partial" blocking response in that either the voltage decrease or frequency increase component of desynchronization was lacking. These responses were also considered normal.

The abnormal responses to flickered light stimulation were divided into the following three groups: I) spike-wave and multiple spikes, four subjects (Figure 2 A–D); II) slowing—delta or theta, three subjects (Figure 2 E–F); III) mixed—sharp and slow or spike and slow, two subjects (Figure 3). All of the abnormal responses considered appeared diffusely over the scalp.

Despite the fact that the patients employed were selected from a larger group on the basis of manifesting a reliable response to flickered light stimulation, consistency varied during the different parts of the study. During the verbal learning part of the study, as shown in Figure 4, the three groups responded with abnormal potentials to "p" light 85 per cent, with normal blocking or driving to "np" light 80 per cent, and with no abnormal waves during the "no light" interval 97 per cent. Group I responded appropriately to the three conditions of light 95 per cent, Groups II and III 89 per cent and 74 per cent respectively. Consequently, the analysis of the data in Figure 4 was performed for those words for which the predicted EEG response occurred.[3] This precluded the possibility that

[3] Analysis of variance was employed to investigate these data in order not to confound possible interaction and main effects. If a statistically significant ($p \leq .05$) source of variation emerged, the individual means comprising it were compared with t-tests.

A table of proportions was constructed in which the denominator indicated the number of paired adjectives within each of the three conditions of flickered light during which the specified EEG response was elicited. The numerator represented the number of correctly learned paired adjectives during those trials. The numerator and denominator could both range from zero to three—appropriate EEG responses to the flickered light and number of paired adjectives learned respectively. Because of the lack of normality of the distribution of these proportions, they were transformed into degrees before the analysis of variance was performed.

This analysis was repeated with another matrix of proportions which was based on all of the words presented, regardless of EEG response to the flickered light. That is, the denominator in all cases was three—the number of words under each condition of flickered light—and the numerator represented the total number of paired adjectives learned under that condition.

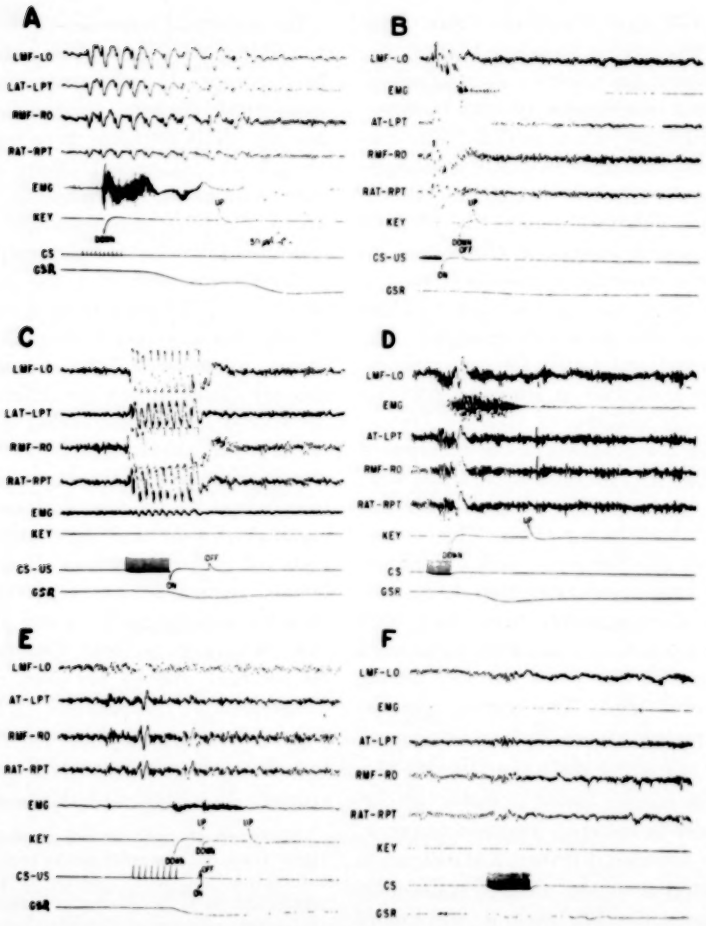

FIG. 2. A. Subject 1. Spike-wave response to flickered light CS with after-discharge. Key-press avoids the buzzer (US). Short latency, high amplitude biphasic GSR.

B. Subject 2. Brief spike-wave burst to the CS. Key-press does not occur during the CS interval. As the CS ends, the US begins and is terminated by key-press. Minimal GSR.

C. Subject 3. High amplitude spike-wave response to the CS. No key-press response occurs and the subject hears the buzzer.

D. Subject 4. Multiple spikes and wave response to the CS. Key-press avoids the US. Moderate amplitude, long latency GSR.

E. Subject 5. Paroxysmal delta response to the CS. Key-press momentarily stopped so there is a brief exposure to the US which is immediately terminated by another key-press. Short latency, moderate amplitude GSR.

F. Subject 6. Theta response to the CS during extinction trials. No key-press occurs so the subject does not experience the US. Minimal GSR containing electrode sway or movement artifact.

a word was learned during a presentation when the expected abnormal EEG response failed to appear; or that a word was not learned because of the appearance of a spontaneous abnormal discharge in the EEG during "no light" or "np light" conditions. The analysis was also repeated for all presentations; *i.e.*, without regard for the EEG response to the flickered light stimulation.

Similarly, the results of the conditioned

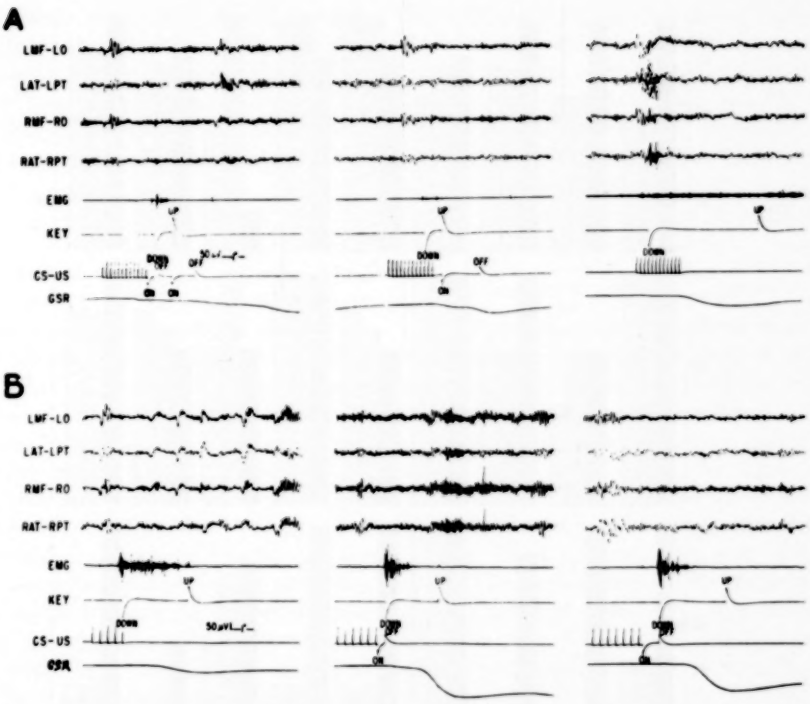

FIG. 3. A. Subject 7. Different EEG responses to the 6 flash per second CS during conditioning trials. (1) Trial #3. There is a sharp and slow-wave response to the CS. Key-press occurs after the US has started and does not last the entire interval. GSR shows a long latency. (2) Trial #5. The EEG response consists of several theta waves. Key-press avoids the US but not for the entire interval. Minimal EMG response. (3) Trial #9. High amplitude sharp and slow-wave response with key-press completely avoiding the US. Movement artifact obscures part of the EEG response. These successive trials show irregular GSR latency and magnitude changes.

B. Subject 9. Different EEG responses to the 4 flash per second CS during conditioning trials. (1) Trial #8. A brief run of moderately high amplitude theta to the CS. The key-press avoids the US. Eyeblink artifact is evident in the EEG tracing. (2) Trial #12. Several sharp waves in response to the CS. Key-press is delayed so that the US is escaped but not completely avoided. (3) Trial #15. Moderate voltage delta and sharp waves. Again, key-press occurs after the CS has started. GSR increases in magnitude but does not shorten in latency in successive trials.

avoidance procedures in Figure 5 are presented both to indicate the reliability of the EEG responses and to permit comparison of performance during trials in which the appropriate EEG response occurred and those in which it did not. During the conditioning procedures the groups responded appropriately to the "p" light and "np" light 74 per cent and 94 per cent of the time. The values for the groups over all the conditioning tasks were: Group I—92 per cent, Group II—88 per cent, Group III—73 per cent. Again, Group I is more reliable than the other two groups of subjects.

Verbal learning: The results of the verbal learning portion of the experiment are presented in Figure 4. The data from Group I is based on three rather than four patients since one patient in this group was not able to recall any of the words in Lists 1 or 2 regardless of the presence or absence of flickered light stimulation of any type. He appeared to be of rather limited intellectual endowment and was also unable to learn

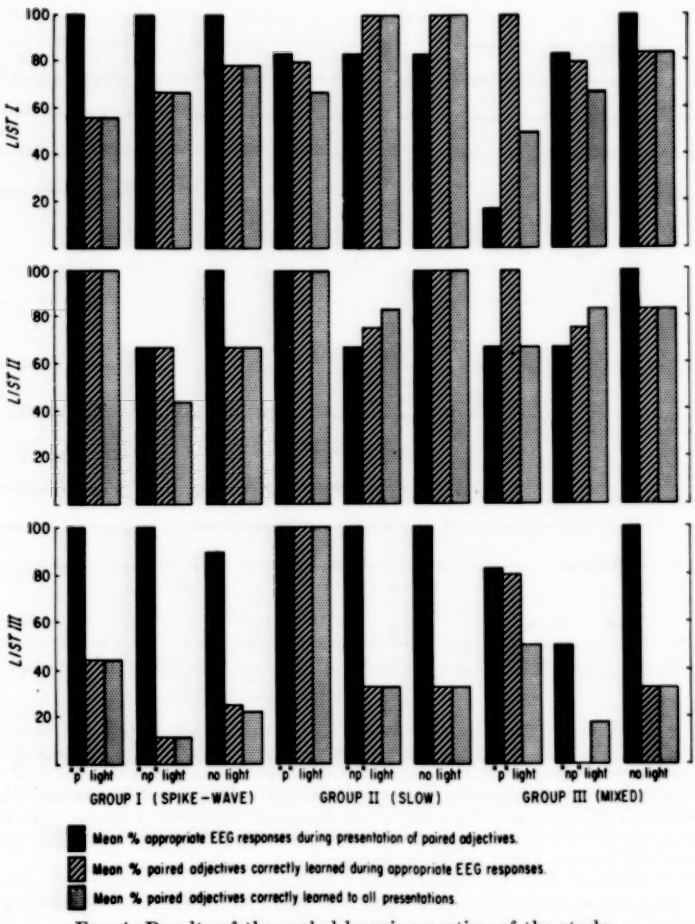

FIG. 4. Results of the verbal learning portion of the study.

any words in a short list of quite easy paired adjectives which was constructed especially for him.

The analysis indicated, as expected, that fewer of the paired adjectives in List 3, the most difficult list, were correctly learned than were those in Lists 1 and 2, regardless of the other variables investigated. Group II (slow-wave) subjects learn more paired adjectives than do Group I (spike-wave) or III (mixed) subjects, when either total number learned or number learned with consistent EEG response is considered. The lack of statistically significant differences between "no light" stimulation, flickered light which evoked abnormal discharge, and light which resulted in desynchronization or driving, indicates that elicitation of an abnormal EEG discharge while presenting paired adjectives has no reliable effect on whether the words are learned. There was also no significant relation between difficulty of the list and interference with learning by the evoked EEG abnormality.

Conditioned avoidance learning: The latencies of the three response measures—key-press (KEY), EMG, and GSR—were measured from the onset of the CS or DS.

The amplitude of the skin change was calculated from the basal resistance (B) and the minimal skin resistance (M) reached in the ten seconds following the onset of the CS or DS. These values were converted to per cents by means of the formula

$$\frac{100 - (B - M)}{B} \times 100.$$

The analysis of the various response latencies indicated that KEY and EMG were significantly correlated and that the latency of the EEG response was shorter than either of the other physiological measurements.[4] GSR tended to become habituated as the experiment progressed and analysis of latency and magnitude of skin resistance changes indicated that neither of these GSR measures were conditioned.[5]

The criterion of learning employed in the conditioned avoidance portion of the study was the ability to make the correct response to avoid the unpleasant US which followed the flickered light signals. The responses during the conditioning, discrimination, and extinction procedures were scored as avoidances if the response prevented the onset of the buzzer for the trial. The various measures of successful avoidance of the US investigated are presented in Figure 5. The analysis performed on these data indicated that among all of these measures of learning, the nature of the abnormal EEG response elicited, the rate of flicker and the order of flicker, made no statistically reliable difference in the number of avoidance responses made.[6] In other words, performance was independent of the EEG response evoked by flickered light and of whether or not any abnormality was elicited.

DISCUSSION AND CONCLUSIONS

The hypothesis being tested in these experiments was that the presence of diffuse spike-wave or certain other EEG abnormalities, induced by photic stimulation in patients with convulsive disorders, would interfere with their ability to learn new material. Verbal learning and the formation of a conditioned avoidance response during induced EEG abnormalities were investigated. The results indicate that the presence of light induced spike-wave, slow-wave, or mixed sharp and slow EEG activity had no effect upon learning ability as measured in this study.

The nature of the abnormal EEG discharge elicited produced a significant difference only in terms of the number of paired adjectives learned. That is, Group II (slow-wave) learned more words than did Groups I and III (spike-wave and mixed, respectively) under all conditions. Formal IQ scores were available for half the subjects in each group and estimates for the remainder did not suggest that the superiority of Group II was due to differences in intellectual level. This, however, deserves further, more systematic investigation.

The results of this study indicate that the type of learning tested was not impaired by the concurrent presence of the elicited spike-wave or slow activity in the EEG. However, although it may not be possible to distin-

[4] This analysis was carried out by first computing product-moment correlations between the latencies, which indicated that only KEY and EMG latencies were significantly correlated ($r = .76$). Following this, the GSR latencies were transformed by dividing by 10 to make the range and the variance over the range more equivalent to the other response latencies. Analysis of variance was then performed on EEG, KEY, and GSR latencies.

[5] The GSR latencies under the various conditions of the experiment were investigated by means of analysis of variance. There were no significant ($p \leq .05$) sources of variation present.

Since GSR magnitude was computed as a per cent, analytic models requiring additivity of scores could not be employed. The Wilcoxon Matched-Pairs Signed-Ranks Test (Siegel, 1956) was used to compare GSR magnitude between the various parts of the conditioned avoidance procedure and the groups of subjects. Again, none of the differences attained statistical reliability.

[6] Analysis of variance was performed on the raw scores upon which Figure 5 is based. None of the main effects or interactions attained the .05 point of significance. This held both for trials with appropriate as well as non-appropriate EEG responses.

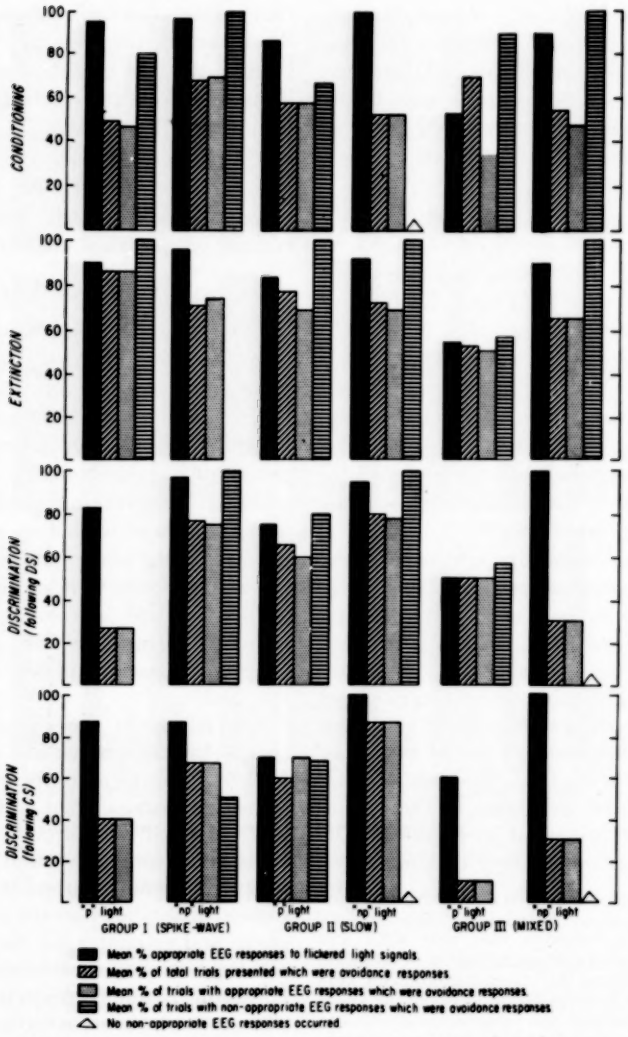

FIG. 5. Results of the various conditioned avoidance procedures.

guish between the appearance of spontaneous abnormal potentials in the scalp EEG and those which are evoked by photic stimulation, they may not be identical. Comparisons of recordings made from scalp derivations and implanted electrodes indicated that seizure patterns may be present in depth and absent at the surface (1), or restricted to the outer cortical lamina (11). These findings raise the possibility that epileptiform activity elicited by intermittent photic stimulation, although appearing diffusely in scalp electrodes, may not involve the "centrencephalic" area or other systems essential to learning. Therefore, the results of this study must be interpreted in the light of the artificial evocation of the abnormal potentials. The brief duration of the evoked

discharges may also be a critical factor. Further investigations are being carried out in which the methods of investigation used in this study are extended to periods of spontaneous abnormal EEG discharge.

SUMMARY

The hypothesis tested in this experiment was that the presence of diffuse abnormal EEG discharges induced by flickered light would interfere with learning new material. Three groups of epileptic subjects were employed presenting the following types of flickered light induced EEG abnormalities: I) spike-wave and multiple spikes, four subjects; II) slowing—delta or theta, three subjects; III) mixed—sharp and slow or spike and slow, two subjects. All of the abnormal potentials appeared diffusely over the scalp.

Two learning situations were utilized. The first consisted of learning paired adjectives of varying difficulty while (a) flickered light which elicited abnormal activity was presented, (b) flickered light which did not evoke abnormal potentials was presented, (c) no intermittent photic stimulation was presented. The second situation involved the formation of a conditioned avoidance response employing flickered light which evoked abnormal EEG activity as the conditioned stimulus. After this response was formed, differentiation employing another frequency of flickered light which also elicited abnormal EEG activity was investigated, as was the extinction of the conditioned avoidance response. Following this, the conditioning, discrimination and extinction procedures were repeated with flickered light at frequencies which did not evoke abnormal EEG discharges.

The results indicated that the presence of spike-wave, multiple spikes, slow-wave or mixed sharp and slow activity in the EEG elicited by intermittent photic stimulation had no significant effect upon the ability to learn paired adjectives, regardless of the level of difficulty, or on the formation of the conditioned avoidance response, discrimination, or extinction.

The possibility is suggested that spontaneously arising epileptiform activity in the EEG may not be functionally identical with flickered light induced potentials of similar appearance. The effect of such "spontaneous" discharges upon similar learning tasks is under investigation.

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COMPARATIVE STUDIES OF THE PHENOTHIAZINE TRANQUILIZERS: METHODOLOGICAL AND LOGISTICAL CONSIDERATIONS[1]

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OVERVIEW

With each new addition to psychopharmacology a cyclical pattern of activity emerges characterized by an initial period of enthusiasm followed by an inevitable period of disillusionment. As empirical evidence is accumulated and analyzed what is usually left is a distillate of clinical experience indicating limited and specific usefulness. Currently the phrenotropic drugs, especially the so called "tranquilizers," are passing from a phase of enthusiastic acclaim to one of realistic skepticism; so also is there a change in the character of evaluative research on these compounds.

Following the introduction of chlorpromazine and reserpine in 1951 and 1952, respectively, the first two or three years of drug research were devoted to testing their relative merits in numerous spade work studies of the type which Feldman has termed "browsing" research (6). Inherent in these early investigations was a search for clues to the parameters of drug action. However, because of widely divergent operational definitions and the lack of rigorous experimental procedures, data from these investigations were not comparable, hence the conclusions derived from them could not be generalized or incorporated in a growing fund of information. Except for the directional quality of these studies, they were of limited value in providing answers regarding comparative drug action.

Since this initial phase of diffuse activity in tranquilizer drug research, the pendulum has now swung in the direction of increased scientific rigor in the assessment of drug action. A general concern with problems of experimental design and analysis has developed, partly as a reaction to the laxity and confusion of former years and partly as a natural consequence of behavioral science advancements. As a result of this concern there has been a substantial increase in the number of scientifically sound investigations appearing in recent drug literature. The development of systematic approaches has also made generalization of conclusions a more defensible procedure.

MULTIDRUG STUDIES

Despite a voluminous drug literature and a general upgrading of the level of drug research, there still remains ambiguity regarding the properties and relative merits of the various tranquilizer drugs that can only be resolved by comparative multidrug studies. While such studies are familiar procedures in other areas of medical research,

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their appearance in the field of psychiatry is relatively recent. As late as 1957, only nine comparative multidrug studies (*i.e.* studies involving more than four psychiatric drugs) had been reported in the literature.

The earliest reported comparative multidrug study carried out in a psychiatric hospital with a psychotic population was that of Pennington (19), who administered six active drugs and a placebo to more than 2,000 chronic patients, with the number of subjects on each drug ranging from 200 to 530. Her study appears to have been a pioneering effort in which a "shotgun" approach was used to assess the value of the various drugs in hospital management of chronic patients. Some effort was made to control extraneous variables such as, for instance, the "placebo" effect. However, from the standpoint of research methodology, the contribution of this study is limited somewhat by the fact that exact procedures were not described.

In 1957 Feldman (5) published the results of an investigation in which he compared five drug categories. This study evaluated improvement on each drug when duration of illness, diagnostic category and certain other relevant variables were held constant. No placebo was used; otherwise every effort was made to maintain stringently controlled experimental procedures. In the case of 200 of the patients studied, a cross-over design was employed with the aim of having each patient serve as his own control. In this same year Margolis (17) summarized problems, opinions, and results in on-going drug research. The author wrote to 80 investigators asking for any information they might have which was not yet published. Observations of drug action were compiled and analyzed for trends in a number of areas, including optimum dosage, side effects, comparative effectiveness of various single drugs, combinations of drugs, and drugs as compared to other therapies.

The remaining five studies did not involve the use of drugs in the treatment of adult psychotic patients: Two publications appeared comparing the effectiveness of five drugs (20) and six drugs (18) respectively, in the treatment of psychiatric outpatients. Bender and Nichtern reported clinical experiences with six drugs in the treatment of disturbed children (2). Kugelmass (11) compared five compounds, including chlorpromazine and reserpine, in the treatment of mental deficiency in children. Himwich, Rinaldi, and Willis (9) compared the effects of five phenothiazine derivatives on reactions to stimuli in rabbits. Chlorpromazine was used as a baseline for these comparisons. Using healthy volunteer subjects, Lehmann and Csank (14) compared a variety of tranquilizing drugs and a placebo as to their effects on the performance of perceptual and motor tasks.

In all of the above studies, with the exception of that of Himwich and his co-workers, drugs compared were not limited to one chemical group but included both rauwolfia drugs and phenothiazine compounds. Though the reports of these authors were of interest insofar as they threw light on the problems encountered in studying several drugs simultaneously, they left much to be illuminated with regard to theoretical principles of drug action. Strictly speaking, drugs of different chemical families cannot be compared, *i.e.*, "calibrated" or placed on a continuum with respect to potency, neurological effect, or any other property. The term "tranquilizing drug" has been criticized because of its vagueness from the chemical standpoint (7). Restriction of the definition of comparative drug studies to studies of several drugs of one chemical family appears to furnish a more useful conceptual scheme, in that it allows us to focus more specifically on the unknown intervening variables between drug intake and drug response.

If consideration of comparative drug studies is limited to those involving more than four drugs of the same chemical family, we find none published before 1959. In 1959 Freyhan (7) reported a comparative study of ten phenothiazine compounds. The setting of

this work was the admissions building of a state hospital and the subjects were acutely disturbed patients selected on the basis of target symptoms which had in common excitement (as opposed to apathy). Though doctors and nurses were responsible for screening subjects and recording independent daily observations of their behavior, the author himself personally saw all subjects initially and made final decisions as to their disposition. Specific difficulties in carrying out this research were not described. Certain controls, such as double-blind administration of drugs and random selection of subjects for the various drug categories were not instituted. This fact, together with the active role which the author himself played in all cases, may have simplified research procedures to some extent. Numbers of patients in the various drug categories ranged from 22 to 69. Freyhan presented in detail data obtained from observation on his particular group of subjects. No attempt was made to test the statistical significance of these findings or generalize to other populations.

Smith, Christian, Mansfield, and Figaredo (21) reported in November, 1959, the results of a comparative study of five phenothiazines. Subjects were chronic patients (63 men, 72 women) who had been refractory to previous treatment. Willingness to take oral medication was one criterion for patient selection. The authors concluded that improvement was so slight on any of the drugs that further study, using double-blind controls seemed unnecessary.

The Department of Medicine and Surgery of the Veterans Administration is in a unique position to gather data on comparative drug action in that investigators can call upon the resources of a number of hospitals located all over the United States. The Psychiatry and Neurology Service of this organization published in August, 1960, the results of a multiple drug study (initiated in May, 1958) involving five phenothiazines and phenobarbital (3). Differences in hospitals were controlled by the stipulation that each drug be represented an equal number of times in each hospital. Each participating hospital was assigned a quota of 18 subjects, newly admitted schizophrenic patients, to be recruited within a period of three months. Responsibility for carrying out research proceedings rested with a staff member of each hospital. Among the difficulties reported by these station investigators were the following: 1) lack of motivation of personnel working in the project; 2) insufficient nursing staff to carry out research activities; 3) modifications of procedures necessitated by idiosyncratic patient behavior; and 4) problems in obtaining a sufficient number of suitable subjects (21).

THE SPRING GROVE COMPARATIVE PHENOTHIAZINE STUDY

After a search of the literature of multiple drug studies both for information relative to drug action and for problems of logistics reported by other investigators, a design was formulated for a comparative phenothiazine study to be carried out at Spring Grove State Hospital. The research was initiated in January, 1958, supported by a grant identified as U.S.P.H. Project MY-2152, for the purpose of comparing the phenothiazine compounds then available on the open drug market in the United States. Preliminary results of the study were reported in June, 1960 (12). Project MY-2152 called for an evaluation of the comparative effectiveness of six phenothiazine derivatives against a baseline of an active placebo (phenobarbital) and an inert placebo (saline/lactose), in short term treatment of acutely ill patients. Phenothiazine compounds to be evaluated were promazine (Sparine), chlorpromazine (Thorazine), mepazine (Pacatal), triflupromazine (Vesprin), prochlorperazine (Compazine), and perphenazine (Trilafon). In planning this investigation our goal was to set up a well controlled, scientifically sound research design which could be carried out in an operational setting.

The specific goals of MY-2152 were as

follows: 1) To determine the differential effects (inter-drug and drug-placebo) of various phenothiazine derivatives on certain fundamental dimensions (factors) of psychotic behavior and symptomatology. 2) To determine the differential effect of various phenothiazine derivatives on the severity of illness as measured by behavioral rating scales and global psychiatric ratings. 3) To determine the differential effect of various phenothiazine derivatives on rate of discharge and other indices of improvement. 4) To determine the relationship between certain case history variables and changes in severity of illness under the various phenothiazine derivatives. 5) To determine the differential effect of various phenothiazine derivatives on certain specific aspects of psychotic behavior as reflected in individual rating scale and check list items. 6) To isolate the biographical and behavioral characteristics which differentiate extreme reactors and non-reactors to the various phenothiazine derivatives. 7) To determine the functional characteristics (e.g. specificity of action, side effect, sedative action, etc.) of the various phenothiazine derivatives.

THE LOGISTICS OF MULTIDRUG RESEARCH

The introduction of a large-scale comparative multidrug study into the setting of a service hospital raises many issues relative both to experimental design and to practical operating procedures. Some of these issues were sharpened in our comparative study MY-2152. Through our experiences in this project—meeting and solving problems in the integration of clinical and experimental realities—we feel we have made progress toward developing new research techniques which are both scientifically respectable and at the same time medically and socially feasible in the setting of an admissions building in a state hospital.

Since we were presented daily with problems of changes in patients' treatment status due to medical and administrative decisions which frequently impinged on our predetermined design, logistical considerations necessarily became a focus of our attention. We believe that the situations we encountered are probably common to most hospitals, and that our pragmatic, on-the-spot solutions may be of some value to others who are planning or conducting similar studies. Procedures have been worked out which permit some degree of flexibility in meeting clinical necessities without unduly compromising research goals. More importantly, our findings suggest that certain "extraneous" factors may in themselves furnish valuable information relative to drug effects. It is the purpose of this section of the paper to focus upon the logistics of multiple drug studies. By the term *logistics*, we mean the network of operations and communications which constitutes research design at the phenomenological level. We hope to identify and analyze factors which might be termed "noise" in multiple drug studies; to report our experiences in dealing with them; and to offer methodological considerations as to their constructive utilization in the overall research program.

In the following paragraphs we shall describe our research design, using as an organizing principle the sequence of operations involved prior to and during the processing of subjects through the study. Commentary on each aspect is intended to furnish information about procedural techniques, to relate each step to the overall research design, and to give some idea of our thinking as to the logical and practical necessity it was designed to meet. Since obtaining a sufficient number of subjects appears to be a major obstacle to multiple drug research, we will present a detailed analysis of the factors which led to loss of subjects at each stage of the study. Information gained from such "drop-outs" proved to be useful, both in terms of indicated changes in our research design and as an additional criterion measure

of drug action. The implications of the latter development for research methodology will be discussed.

STAFF COOPERATION

Carrying to successful completion a necessarily involved study such as MY-2152 requires the whole-hearted participation of clinical personnel of all levels at every step of the operation. The degree to which such cooperation can be obtained is variable. This variability is closely linked to the communications system developed between clinical staff and research personnel and the emotional climate engendered and sustained. Because medical and social responsibilities must be maintained, it is necessary to allow the physician's judgment to take precedence over research procedures in matters directly involving patient welfare. Clinical observations are the facts with which we must work in evaluating treatment effects. Thus a crucial factor in achieving clean data and in maintaining a smooth operating procedure is the establishment of a cooperative working relationship with ward personnel.

Prior to the data-collection phase of Project MY-2152, a series of orientation meetings was held for the physicians treating the patients on the acute admissions service. These discussions covered the following topics: The Psychopharmacology of the Phenothiazine Derivatives, Neurophysiology and the Tranquilizing Drugs, Current Clinical Practices with Phenothiazines, and the Research Methodology of Project MY-2152. During the orientation period, written information regarding the chemistry and psychopharmacology of the phenothiazine derivatives to be used in the study was distributed to physicians on the admissions service. Along with this information a protocol of the experiment was also issued. Each physician was then seen individually by the project director to insure that everyone involved in the treatment of project patients would have a thorough understanding of his role in the project. It was emphasized during these meetings that participating physicians could consult with the project director whenever they had questions or anxieties regarding the study, but that they alone were responsible for the selection of the patients to be placed on the phenothiazine tranquilizers and for the subsequent handling of these patients during the course of medication. The only restriction placed on them was the fact that the medications would be determined randomly and be administered in a double-blind fashion.

Despite these efforts, misunderstandings and failures with regard to subject recruitment and physicians' handling of drugs occurred. Here certain interpersonal "noise" must be taken into consideration. Although it is the hospital's policy that each physician, even the first-year resident, is in charge of his patient's treatment, there were instances in which physicians felt that the Phenothiazine Research Project was imposed upon or handed down to them, and they exerted their independence by not referring patients. Some of the physicians referring patients to the project did not consider themselves as part of the research team since they had not participated in planning the study and had very little opportunity to offer criticism. On a busy admission service with many rules, regulations, and by-laws to be learned by the new employee-physician, the specifications of the Phenothiazine Project were an additional burden. Also, a certain amount of covert resentment was caused by restrictions on the prescribing of medication. The physician's giving up this "right" was not offset by sufficient participation in planning and discussing the Phenothiazine Research Project, at least in the early months of the study.

In order to maintain maximum support on the project, the chiefs of service on both the male and female admission wards were invited to participate directly. Since their duties consisted of making daily rounds on

all the patients on their services, it was not too difficult to obtain their cooperation in working blind and preparing global evaluations and ratings of target symptoms on each patient. Besides their regular duties as clinical consultants, the chiefs of service acted as liaisons between the ward physicians and the research team; whatever criticisms or dissatisfactions these psychiatrists presented to their chiefs relative to the project were given immediate attention.

Since ward observation scaling devices were being utilized in the project, a nursing aide on each of the admission wards was designated to make these observations and was compensated on a fixed basis for the extra effort and time involved. It was specified that research activites (filling out ward behavior scales, dosage forms, etc.) were additional duties to be performed on her own time. Recognition of ward research duties as a part-time job with its own fixed requirements as to time and competence proved to be quite helpful in recruiting and maintaining a dependable corps of ward observers.

Among hospital personnel the chiefs of service, one nurse on each ward, and the pharmacist were directly involved in research activities. Both formal and informal communications from these sources came directly to the project director's office located in the admissions building. In practice all research personnel, in addition to keeping open these specified channels of communication, came to function somewhat like small-town newsmongers. They developed a "nose" for what was happening to patients: why a a given subject was dropped from the project; what action was about to be taken which might contaminate data, etc. Generally speaking, data collection involved knowing intimately hospital routines and staff idiosyncracies; taking nothing for granted; and keeping one's nose always to the wind.

SUBJECTS

As each newly admitted patient was assigned to a ward physician, the physician made a decision as to whether or not the patient was to be placed on a tranquilizing drug. The basis for his decision was the presence of target symptoms such as hyperactivity, anxiety, tension, overt aggression, etc. Each doctor was supplied with criteria for selection of research subjects: any newly admitted patient over 18 years of age having the target symptoms described, with the exception of seniles, alcoholics, court orders, chronic brain syndromes, and major organic diseases. If the physician decided to place such a patient on a tranquilizing drug, he was requested to notify the research department and allow two days to elapse in order that the department might obtain the necessary evaluations and assign and provide necessary medication for the patient on the ward.

At the time that Project MY-2152 was developed, we had no information as to the effect of sedatives on psychotic symptoms. Thus we felt that it was necessary to withhold all medication for the two days of preliminary observation. To an extent this stipulation resulted in the loss of a type of subject who would be expected to respond well to phenothiazine drugs: the hyperactive, disturbed patient for whom the physician felt immediate treatment was necessary. This procedure, in effect, constituted a selective factor in the sample of patients finally secured. An analysis of admission records during the first 18 months of the intake of research subjects revealed that 133 patients who appeared to be suitable for the project had not been referred. Where possible the physicians involved were questioned as to their reasons for not placing these patients in the study. Table 1 presents an analysis of the reasons given by doctors for not referring subjects.

It will be noted that almost 50 per cent of the potential subjects were not referred be-

TABLE 1

Potential Subjects Not Referred—Doctors' Reasons

Patients too disturbed*	56
On tranquilizing drug when admitted	9
Chronic patient—to be transferred shortly	9
Not familiar with research procedure	3
Mistake	3
Admitting physician's medical judgment	15
Information not available	38

* The majority of these patients were weekend admissions who would probably have been referred routinely had they been admitted during the week.

cause they were considered too disturbed. As indicated, for the most part these were patients admitted on weekends, when service coverage was at a minimum, who probably would have been referred to the project routinely had they been admitted during the week. Physicians who gave "already on tranquilizing drugs" or "chronic patient" as their reason for not placing patients on the study frequently explained in their notes that the patient had reacted well to a given drug in the past, so they prescribed it again. It was on that basis, rather than on the basis of any specific research criterion, that such subjects were withheld from the study. It might be said here that our orientation program did not include doctors not assigned to the admissions building who, nevertheless, occasionally prescribed medication on an emergency basis. Communication with these physicians was thus inadequate, with some resulting detriment to research procedures. Also, many physicians serving in the admissions building were residents; hence, there was almost a complete turnover each year. Steady growth of understanding was difficult under such circumstances.

CRITERION INSTRUMENTS

Following the identification of a patient for drug treatment a set of criterion measurements was secured. During the first two days (before any medication was administered) each subject was rated by psychologists, ward personnel, and a research psychiatrist on a series of behavioral rating scales as follows: 1) *Psychiatric Rating Scale*, completed by the research psychiatrist. This was an instrument in standard rating scale form by which patients were evaluated as to overall adaptability and discomfort, as well as the extent to which they manifested target symptoms. 2) *Multi-dimensional Scale for Rating Psychiatric Patients* (MSRPP) (15), completed by a clinical psychologist on the basis of a one-hour interview and a nursing aide on the basis of ward behavior. 3) *Psychotic Reaction Profile* (PRP) (16), a checklist of ward behavior completed by the nursing aide.

Inter-rater reliability had previously been found to be satisfactory for each of these instruments. Ratings were blind, in that raters did not know which drug the patient was receiving. Patients were evaluated on the MSRPP scales at the end of two weeks, four weeks, and six weeks of treatment. The PRP and the psychiatrist's ratings were completed each week for a six week treatment period.

DRUGS

Drugs were administered to the patients on a double-blind basis. The choice of the particular phenothiazine (or placebo) to be used in any case was based on a predetermined random selection from the various drug categories. Physicians were aware that the patient was receiving either a tranquilizing drug or a placebo (with the odds 3 to 1 in favor of the former), but they had no information as to the particular medication of any patient. The dosage schedule for each of the eight drug categories is given in Table 2; parenteral dosages were those in standard use and oral dosages were those recommended by the respective pharmaceutical companies as being generally applicable over large inpatient populations.

For the first two days of medication the drug was given parenterally. Drugs were

TABLE 2

Comparative Dosage for Phenothiazine Study

Drug	Parenteral (1st 2 days)	Oral (3rd day) Minimal Dosage*	Capsule Dosage*	
			#1	#2
Promazine	50 mg./cc	300	100	200
Chlorpromazine	25 mg./cc	300	100	200
Mepazine	25 mg./cc	75	25	50
Trifluprom azine	25 mg./cc	75	25	50
Prochlorperazine	5 mg./cc	30	10	25
Perphenazine	5 mg./cc	24	8	16
Negative Placebo Isotonic Saline	1/cc		Lactose	
Positive Placebo Sod. Phenobarbital	65 mg./cc	97.5	32.5	65.0

* All dosages expressed in milligrams.

provided in ampules containing 1 cc. and the dosage was 1 cc. given intramuscularly T.I.D. Parenteral medication was given to insure that each patient received medication and to allow for the most effective build-up of the medication in acutely disturbed patients. On the third day oral medication was started. The dosage schedule at this point called for a minimum of three capsules a day, a schedule which provided in all cases the minimal dosage level recommended by the respective drug companies. The physician was at liberty to increase the dosage to the point which he felt to be the most therapeutically effective. He could prescribe as many of the #1 or #2 capsules as he thought necessary with no predetermined ceiling. The physician could discontinue the medication when and if he felt that he was not getting the desired effect. If a patient had remained in the study for a minimum of ten days, he was reevaluated on criterion measures when experimental medication was discontinued. The research psychiatrist participated as a consultant in any decision which involved removing a patient from the study; hence the research department was advised immediately of such an action. Whenever physiological complications due to the drugs developed these were also investigated and recorded.

The final cut-off point in the observation of any particular patient was at the end of six weeks. If the patient was still in the hospital and on drug medication at the end of this period of time, the ward physician could continue the patient on the same medication for an extended period of time if he so desired but the patient was no longer observed for rating purposes. Upon request, information as to the particular drug a patient had received was made available by the pharmacist to the physician after completion of the six week period.

Unanticipated problems arose regarding the implementation of the planned procedures just described. For example, ward physicians, mostly first year residents, had difficulty in accepting the "huge" doses of tranquilizers prescribed in the protocol. Most of these residents had just finished their internships and had been accustomed only to small doses of tranquilizers. It took time for them to adjust themselves to large doses of known tranquilizers; increasing the dose of an unknown medication proved even more threatening to them, not altogether without justification. A somewhat legitimate complaint against increasing the medication and reaching high levels was the fact that the "margin of safety" or "therapeutic range" was widely different among the various phenothiazines used in the project. (Illustrative of this is that three times the minimal dose of perphenazine and chlorpromazine are 72 mg. and 900 mg. respectively; 72 mg. of perphenazine a day would be expected to produce neurological complications in a great number of patients; whereas 900 mg. of chlorpromazine would not.) Another reason for reluctance to administer high doses, or failure to increase medication whenever necessary, was the insufficient coverage by medical personnel on evenings and weekends—only one physician being on duty for a 2,600 bed hospital. This was coupled with the fact that evening personnel consisted mostly of attendants with very little nursing training who could

not be expected to recognize quickly the appearance of neurological and other complications. For these and other reasons most physicians hesitated to increase appreciably a patient's medication, especially before a weekend.

It took much time and repetition to insure that the resident physicians understood that the routine doses were the minimum recommended and not the standard ones for the drugs involved. Inability to see the patients frequently in order to check their progress is only a partial explanation of the doctors' reluctance to increase dosages. The lack of knowledge concerning the treatment of neurological complications was another reason. The dramatic nature of neurological symptoms was alarming to inexperienced physicians and ward personnel and resulted in the unnecessary removal of patients from the project.

Understandably, the physicians we were dealing with had difficulty familiarizing themselves with the actions of the various drugs involved. Thus, many patients were removed from the project and placed on chlorpromazine, "something that is going to work and something that we know." There was also pressure on physicians from ward personnel "to give something that will work." This was especially true of evening personnel whose shifts were at times inadequately manned to care for very disturbed patients. Frequently a patient was taken off the project a few hours after referral for the above reason. Only with persistent consultations did our physicians learn to recognize a good candidate for treatment with tranquilizers and to understand that the behavior of highly disturbed patients frequently represented target symptomatology which would yield to drug therapy.

There were other problems on the nursing level. Quite often the patients in the study, or their relatives, questioned the nursing personnel regarding the identity of the medications being administered. The standard reply agreed upon was that the patient was on a "phenothiazine tranquilizer." This seemed to gain universal acceptance, with little further questioning. However, on a few occasions the project director had to placate an irate physician or dentist, not connected with the project, who had received such a reply by apologetically revealing the specific drug involved. Admittedly, "ruffled feathers" were with us from beginning to end in spite of our efforts at noninterference.

Also, at the beginning of the study, a few of the nurses indulged in guessing games to determine the medication being administered, particularly whether or not a placebo was involved. These extraneous machinations quickly disappeared, however, as the novelty of research procedures wore off. A matter-of-fact attitude soon prevailed as the nurses realized that they were faced with an increased responsibility for the handling of medication since all the drugs looked alike. The problem of patients hiding the drugs was encountered in spite of careful checks to prevent this. At first, patients who refused to take oral medication presented the nursing service with obvious difficulties until it was decided, by a reluctantly compromising research staff, that the medication could be mixed with liquids or foods.

PATIENT MOVEMENT—"DROP-OUTS"

The flexible plan of MY-2152 described above was originally considered a necessary modification (and possible contamination) of pure research design. As it turned out, incorporating the physician's medical judgment in planned procedures led by serendipity to a methodological approach which we feel has some promise. This same flexibility, however, left us more than ever with the problem of subject attrition, a major obstacle to multiple drug studies or to any medical research using human subjects in which various types of treatment are compared. In the following paragraphs, the implications of the various types of patient

TABLE 3
Factors Responsible for the Termination of Treatment

Factors	No. of Pts.	Percentage
1. No response or worse (according to ward psychiatrists)	81	29
2. Transfers to other services	44	16
3. Transfers out, paroled or discharged	35	13
4. Drug reactions	27	10
5. Organic conditions not induced by drugs	23	8
6. Medication no longer needed	8	3
7. Total number terminated	218	79
8. Completed 6 weeks,	59	21
9. Total referred	277	100

movement as applied to the analysis of drug action will be discussed. Statistical analyses of drug action in terms of criterion data have been performed and full results will be presented in subsequent papers. Our interest here is in the utilization of information obtained from various types of patient movement which appear to be inescapable facts in any hospital setting. Table 3 presents the factors responsible for the termination of treatment of patients in the drug study before the end of the projected six weeks of treatment.

The amount of patient movement is immediately evident from the table. Whereas we had expected an 80–20 per cent breakdown of completion *vs.* terminations, actually the reverse was true. Apparently six weeks of continuous treatment with any one drug is an unrealistically long period in an admissions setting where emphasis is upon prompt alleviation of acute symptoms and rapid processing of patients. Also, it was our experience that for the majority of patients the amount of symptom alleviation with the drugs was substantial the first two weeks, so much so that we obtained significant differential results on our criterion measures at the end of this time. After this initial leveling off had occurred, alleviation during subsequent weeks was relatively slight. On analysis, it was apparent that many patients in the "no response" group of Table 3 had actually shown some improvement over initial conditions but that this improvement was not rapid enough for ward psychiatrists intolerant of slow or minimal responsiveness in their patients.

A breakdown by drug category of the 81 subjects who were taken off medication because of no response or worse revealed an interesting relationship. There is a rank-order correlation of $-.96$ (significant at the .01 level) between the number of patients in each drug category removed from the study and the potency of the drug they were receiving. In the case of the two placebos, phenobarbital and saline/lactose, both showed more patients removed because of no response or worse than did the active drugs. Phenobarbital ranked as high as the inert placebo in rating scale data, as well as in number of patients showing no response or worse, probably because of some increase in paranoid belligerence in patients given this drug (as indicated by staff ratings). The actual number of "no response" patients in each category was: phenobarbital 15, placebo 15, promazine 12, mepazine 11, chlorpromazine 9, prochlorperazine 7, triflupromazine 7, and perphenazine 5.

Table 4 presents the incidence of neurological and other somatic symptoms by drug category for the 27 patients (approximately 10 per cent of the total) whose side reactions resulted in the termination of treatment. The number of days on medication, the daily dosage at termination, and the particular type of reaction occurring are presented. All but one of the 14 neurological complications occurred with the three most potent drugs, whereas 11 of the 13 other somatic reactions occurred with the less potent ones. Discounting reactions with placebos, the probability under the null hypothesis of the distribution of frequency presented in Table 4 is $p<.01$ (Fisher's Exact Probability Test). Eight of the 36 perphenazine patients developed neurological reac-

TABLE 4

Side Reactions Causing Termination of Treatment*

Drugs†		Reactions													Number	
		Neurological								Other Somatic					Neur.	Other
Perphenazine (36)	Type	D	P	D	D	D	D	A	P							
	Dosage‡	15	15	15	15	15	5	16	68						8	0
	Day	2	2	2	2	3	3	10	14							
Prochlorperazine (32)	Type	P								V	R					
	Dosage	75								30	60				1	2
	Day	17								14	21					
Trifluopromazine (36)	Type	P	D	D	A											
	Dosage	75	75	25	150										4	0
	Day	1	2	5	23											
Mepazine (34)	Type									S						
	Dosage									450					0	1
	Day									28						
Chlorpromazine (33)	Type									L	V	R	V	R		
	Dosage									300	600	400	800	200	0	5
	Day									6	10	17	17	22		
Promazine (32)	Type	D								V	V					
	Dosage	400								150	800				1	2
	Day	10								1	29					
Phenobarbital (37)	Type									V						
	Dosage									180					0	1
	Day									14						
Placebo (37)	Type									G	R					
	Dosage									6	12				0	2
	Day									12	13					
															14	13

* Neurological: Parkinsonian (P); Dyskinetic (D); Akathisia (A)
Other: Leukopenia (L); Vasomotor (V); Visual (S); and Gastric Disturbances (G); Rash (R)
† Presented in order of potency; number of patients referred given in parentheses.
‡ Daily dosage at termination of treatment; placebo dosage in terms of frequency of #1 capsule.

tions, accounting for the majority of patients in whom such reactions occurred, and of these, six developed reactions while on parenteral medication. Three of the four trifluopromazine patients also developed neurological reactions while on parenteral medication. (For some of these patients the oral phase had purposely been delayed because of suspected neurological involvement.)

In current drug research increasing recognition is being given to the significance of administrative decisions affecting a patient's status as indication of degree of illness. Illustrative of this trend is a recent attempt to assign numerical scale values to those decisions which have been found to be of general, wide-spread occurrence and applicability (13). However, in this present study the utilization of transfers, paroles, discharges, etc., as objective criteria of drug responsiveness proved somewhat disappointing. So many of these dispositions were influenced by environmental pressure, family demands, etc., that the meaningfulness of these measures had to be questioned. Another obfuscating element, as far as these criteria were concerned, was the nature of the illness of the patient. When such characteristics as type, degree, and chronicity of illness were considered in an analysis of drug effects on hospital dispositions, no differentiating results were obtained. This need not have been the case had such dispositions been systematically categorized and controlled in terms of pathology rather than determined by the exigencies of the situation.

COMPARATIVE PHENOTHIAZINE STUDY: SECOND PHASE

Currently we are engaged in a comparative study of eight phenothiazine derivatives, including four of the most effective drugs as determined by our first study (chlorpromazine, triflupromazine, prochlorperazine, and perphenazine) and four of the newer compounds: fluphenazine (Prolixin; Permitil), trifluoperazine (Stelazine), thiopropazate (Dartal), and thioridazine (Mellaril). Essentially the design of the study is the same as Project MY-2152, except for a few changes made as a result of our experiences in the former project. The logistics of our approach has also remained relatively unaltered. Changes in the research design will be discussed briefly, since they illustrate further our attempts at solving problems at the operational level.

The first important change in the second phase is the dropping of a placebo control. As a result of our findings in MY-2152 demonstrating significant drug effectiveness beyond that achieved with placebos, and in view of the preponderance of evidence in the literature on phenothiazine drug activity, we have concluded that a placebo control in our setting is no longer an essential element of design. Especially is this so in view of the relatively unchanged character of our treatment and data collection procedures. The elimination of placebos from our treatment categories has obvious implications in terms of staff cooperation, fewer "drop-outs," etc.

The second major change is the provision of routine sedation for the patients' first two days on the project. This change is regarded as the not wholly satisfactory but probably most nearly feasible solution to several knotty problems of logistics. Our initial plan of giving the assigned experimental drug parenterally to each patient had to be discarded because thiopropazate and thioridazine were not available in parenteral form, and there was little prospect that the respective drug companies would produce a parenteral form of these drugs in the immediate future. By substituting a two-day period of sedation for the two-day waiting period of MY-2152, we hope both to retain the more disturbed patients and to sedate them to the extent that drugs can be given in oral form at the end of this period.

Since results of MY-2152 indicated that little information would be lost if patients were studied for four rather than six weeks, we planned as a third major procedural change to run this study on a 30 day cut-off basis. For most of our patients, significant changes in pathology occur in the first two weeks; by the fourth week patients usually reach a point beyond which very little, if any, further change is noted. Evidence of this fact is the large percentage of our patients (20 per cent) who were dropped from our first project after the fourth week.

Although the ward physicians still maintain ultimate responsibility for their patients, the current requirement is that patients be kept on the project for five days to insure at least a minimal trial for each medication. If no improvement occurs by the fifteenth day in the type of patient with which we are dealing, we feel safe in assuming that little additional change will occur in most cases, and only minimal information would be gained by keeping the patient on the experimental drug for a longer period of time. On the fifth, fifteenth, and thirtieth day the patients are evaluated in terms of criterion instruments, including the *Minnesota Multiphasic Personality Inventory* (8) and the *MACC Behavioral Adjustment Scale* (4) in addition to those already described. These days have been selected as a result of our findings as to the period of time during which drug action usually becomes apparent in the acutely disturbed patient. Also, our previous experiences have indicated that our physicians tend to make decisions at these intervals. By focusing on such specified periods of time, it is felt that more meaningful information about patients taken off of their assigned experimental drugs can be

gained in determining characteristic drug reactivity.

DISCUSSION

Experimental design, as usually conceived, consists of actively manipulating reality to meet the experimenter's purpose. Where this is not possible because of social or humanitarian considerations, statistical techniques serve as a not-too-satisfactory substitute. Medical research can make use of animals in controlled experimentation. However, psychiatric research must usually concern itself with the facts of consciousness, intangibles from the standpoint of our present knowledge. Cultural sanctions being what they are, treatments of sick individuals cannot be patterned as rigidly as can types of fertilizer used on patches of sorghum. Uncontrollable conditions are built into drug research with human subjects. Like original sin, these annoying loose ends are part of reality, whether we like it or not. Scientific chastity may be maintained by stating categorically, "This ought not to be," when human factors interfere with predetermined design. This attitude puts us on the side of the angels, well within the orthodoxy of scientific method as now conceived, but contributes little toward a solution of the problem.

It is disconcerting to realize that, in scientific disciplines dealing with human life, "facts" may not be conceptual elements, so to speak, but irreducible complexities, so that we cannot properly identify the variables which we wish to control. The most carefully thought out research design may, in the process of execution, reveal fallacious underlying assumptions or an inappropriate level of abstraction in its conceptual framework. Some of our problems in research with human subjects may be a function of refusing to come to terms with this kind of ignorance.

Let us examine the presuppositions underlying the research plan of MY-2152. In selecting a period of six weeks for evaluating drug action, we were proceeding on the basis of information gleaned from studies of chronic patients, because no data were available on response to phenothiazine drugs in acute patients. Actually, our interest was in changes in psychotic symptoms whenever changes occurred. Data from the study indicated that whatever improvement took place generally occurred within the first two weeks of treatment. If a patient improved sufficiently during this period to be discontinued from treatment (thus becoming a "drop-out"), this is a fact which is directly relevant to drug evaluation. Similarly, a transfer to a convalescent cottage or to a chronic building may very well indicate a change for better or for worse in his illness if proper precautions are taken beforehand to eliminate extraneous determiners of such dispositions. It is a kind of scientific tunnel vision which prevents us from capitalizing on such information. Such patients may be "drop-outs" from the standpoint of a subject population who must receive a given drug for the arbitrarily determined six week period. However, they need not be "drop-outs" in the sense of yielding no data on drug action. We do not at present know enough about the phenothiazines or the individual patient's reaction to them to set up a design in which dosage and length of time of medication are rigidly fixed. We do not know the rate at which these drugs are metabolized by individual patients, nor what qualities differentiate drug or placebo reactors from non-reactors. In view of our ignorance of such matters, it appears that we must learn to live with uneasy flexibility, adhering to classical research design insofar as possible, but not failing to examine events which may be "noise" or extraneous clouding distractions in that frame of reference for pertinence to the subject of inquiry.

SUMMARY

This has been a report of the background and execution of a controlled comparative drug study of six phenothiazine compounds and two placebos carried out with acutely ill patients in a state psychiatric hospital. Problems of methodology and logistics in-

volved in planning and implementing such a project have been discussed. Research design has been examined at the phenomenological level as a complex of actions, decisions, and communications among hospital-clinical staff, research staff, and patients. From our experiences we have concluded that a research program can be scientifically rigorous and yet sufficiently flexible to encompass and make use of clinical and administrative realities.

Well-controlled objective studies of the comparative efficacy and specific properties of phenothiazine compounds are sorely needed to provide adequate drug selection criteria for the practicing psychiatrist. It is hoped that this investigation may furnish a partial answer to the question of feasibility of large scale research of this type in an operational setting. In addition to the significance of our findings with respect to drug action (which will be reported in more detail in subsequent papers), we feel that our research program might be considered a pioneering effort both in design and in execution. As such we hope it may serve as a guide for future projects in the field of psychopharmacology.

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THE LEVELS OF THE NERVOUS SYSTEM AND THEIR CAPACITY TO FUNCTION INDEPENDENTLY OF EACH OTHER

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Two men are reading silently. The face of one is at rest, while the other is moving his lips, forming each word as he reads it. What is the meaning of this difference?

The first man is the luckier. He is the more advanced in his evolution, if only in the one respect that he can read without moving his lips. It is a common expression of contempt to say of a man, "He can't read without moving his lips."

This inability to read without moving the lips is a minor problem in human behavior. No one ever consulted his physician with this as a complaint, and no foundation will ever endow a research fellowship to find a cure for it. But like so many small signs of big events, it throws some light on an important chapter in brain physiology.

The phenomenon, or phenomena like it, may occur in people otherwise of high calibre. A woman of superior intelligence reported two points of interest. 1) She is a slow reader; though she reads without moving her lips, she feels she "says" each word in her throat, and she thinks this is why she reads slowly. 2) She is an accomplished singer, with impeccable pitch. She has always noticed that whenever she hears someone sing a note off pitch, she has a cramp-like sensation in her throat.

This cramp-like sensation clearly indicates that she is singing the correct note silently. When we hear a note sung off pitch, we instinctively think of the true note, the one that *should* have been sounded. This woman not only *thought* of the true note; she *sang* it, albeit silently. Golla (3), recording the movements of his own larynx, found that when he sang an octave he obtained an ascending curve, and when he merely thought of the notes of the octave he obtained the same curve, though of smaller amplitude.

It is clear that mentation can be accompanied by muscular action, even when such action is not intended. In extreme cases it is accompanied by forced audible speech, as in the case of a school teacher of 37 who emerged from a catatonic stupor and became for a time very nearly normal mentally. She recalled that the four days that followed her emergence from the stupor were "horrible." In these four days she was overtalkative. "I had no speech control. It seemed as if I couldn't *think* things, but could only *say* them. There was a great fear in my mind that I would never again be able to think without speaking, and I thought how horrible it would be to have to go through life that way."

All these phenomena can be understood in the light of some well known facts of infantile mentation. The child "thinks with his muscles," including the muscles of speech —a type of behavior that persists, if only in rudimentary form, in the adult. More specifically, it persists in rudimentary form in the healthy adult. Under pathological conditions its persistence may be more than rudimentary.

A good example of the role of muscle in mentation occurred in the case of a healthy infant who automatically responded to an airplane passing overhead by looking up at it and reaching for it. He also reached for it when he was indoors and heard a plane which he could not see. The incident occurred when he was twelve months old. He was not yet able to stand without support. He was standing at the bookshelf, holding on with one hand, while with the other he was tossing some magazines one by one to the floor, a

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favorite pastime of his. Presently he heard a plane. Still holding on with the one hand, he reached aloft with the other, maintaining this posture until the plane had passed out of earshot, when he resumed his play with the magazines. His image of a plane included a motor component: one reaches up for it.

This lad grew up to become a jet pilot in the U. S. Air Force. He no longer reaches up to try to touch a plane in the sky, but I venture to think that whenever he sees or hears a plane, or even thinks of one, action currents still race down the nerves to his shoulder muscles.

The role of overt movement in mentation is seen more especially in the domain of speech. The words in which the young child thinks are overt; they are spoken rather than silent. There is a lesson in the remark of the little girl quoted by Brain (1) who, when admonished to think before she speaks, protested, "But how can I know what I think till I hear what I say?"

The ability to think silently, to play one's cards close to the chest, to think without tipping one's hand, is a trait of the adult, not of the child. The words in the child's mind automatically roll off his tongue. An example occurred in the case of the youngster above, who used to reach for the planes. When he was five years old he and his mother went shopping; they bought a handbag as a birthday gift for the maid. It was to be a surprise, and the mother cautioned the boy to say nothing to the maid. On arrival home the boy promptly told the maid, "We bought you a birthday gift." The following conversation ensued (all in the maid's presence). *Mother:* "Oh, David, we were going to *surprise* Eula." *David*, realizing his blunder and trying to make the best of a bad position: "Well, Mother, I didn't tell Eula we bought her a *handbag*. I only said we bought her a *gift*."

The critic Cowley (2) has recorded some surprising and fascinating observations made by creative writers on the relation of hand movement to literary production: "Hemingway used to have the feeling that his fingers, as they held a pencil, did much of his thinking for him. After an automobile accident in Montana, when the doctors said he might lose the use of his right arm, he was afraid he would have to stop writing." It is amazing—and instructive—that the artistic labors of a Hemingway should involve so large a motor component.

Since mentation is a function of the highest levels of the brain, while speech and other movement involves action of the lowest levels (motor centers in bulb and cord that innervate muscles), we are led into the problem of the relation of the various levels of the nervous system to each other. Though the number of levels is indefinite, it will be convenient to speak, like Hughlings Jackson, of three: the lowest, the middle and the highest.

On the motor side the lowest level, as conceived by Jackson, consists of motor centers in medulla and cord, centers that represent muscles most directly. The middle level consists of the centers in the precentral gyrus; these "re-represent" the muscles. The highest level consists of centers anterior to that gyrus; these "re-re-represent" the muscles. On the sensory side there are corresponding levels.

Jackson regarded the brain as a mechanism for the "coordination of impressions and movements." (By "impressions" he meant impressions made on receptor organs.) The brain, in other words, is an instrument of adaptation, enabling the organism to respond, or move, in a way suitable to external conditions. The lowest level mediates responses of the simplest type, as when you withdraw your hand from a hot stove. The highest level mediates the most complex responses, as when you reply to your opponent's move in a chess game.

Jackson (following Herbert Spencer) argued that the neural action that underlies all imagery is not just sensory but sensori-*motor*. Thus when you look at a brick there is not only sensory action, corresponding to

the perception of the brick's color, but also motor action, for your eyes move to take in its outline. Elsewhere I have quoted at length Jackson's views in this matter (5).

Jackson distinguished "vivid images" and "faint images." When we see a brick we have a *vivid* image; there is action of all three levels (on both the sensory and the motor sides). When we merely think of a brick we have a *faint* image; there is action of the highest sensory and highest motor centers only, or mainly.

Since imagery involves both sensory and motor action, Jackson considered the highest cerebral centers to have a "reflex constitution." The "organ of mind" is an organ of reflex action, as much so as any spinal arc. It differs from spinal reflex mechanisms only in being infinitely more complex. This is a view that has been rejected by many (5).

We return to the imagery of the infant. As we see from the example of the infant who reached for planes overhead, there is muscular action in imagery. A plane is something you not only hear and see (activities which in themselves involve motor action); it is something high up in the sky that you reach for. As the infant grows older, he no longer reaches for planes. Now when he sees a plane (vivid image), there is activity of all three levels. But when he merely thinks of a plane (faint image), there is activity of the highest level only—save insofar as there might be rudimentary action of shoulder muscles without overt lifting of the hand.

Enough evidence has been presented to show that the infant, in thinking, uses his muscles—overtly at first, and, as he grows up, implicitly. As he grows up, his highest cerebral centers develop the capacity to function, when necessary, "independently" of lower centers.

These are substantially Jackson's views, which he expressed as follows (4, volume I, page 375):

"Another way of regarding the evolutionary process is to say that the several levels, in spite of their dependence on one another, attain, as evolution progresses, a degree of independence of one another.

"As evolution progresses the highest centres not only gradually develop (become increasingly complex, etc.), but also become more and more detached from, and more independent of, the lower centres out of which they have been evolved. . . . There are degrees of detachment and of independence. . . . Our highest sensory and highest motor centres (together the 'organ of mind') can energise, to a large degree, independently of the lower centres out of which they have been evolved, and by aid of which they have been developed; consequently they can act independently of the environment."

When evolution is incomplete, the highest cerebral centers remain in some degree dependent on lower centers, requiring their "assistance," as in the case of people who move their lips as they read. And the same thing is seen in disease, as in the case of the catatonic cited above, who for several days "had no speech control" and had to speak her thoughts.

INDEPENDENCE OF LOWER CENTERS

Thus far we have been concerned with the independence of the highest centers. Being at first dependent on lower centers, they in time achieve independence. The corresponding statement applies to lower centers. These begin by being dependent on centers above, and in time they too achieve independence.

Lower centers act independently during behavior that is automatic. Much behavior is automatic, taking place "without thinking." Two people engaged in conversation while walking together are walking automatically. They pay no attention to the movement of their lower limbs, but attend only to their conversation. Lower centers for locomotion can carry out their task automatically, without direction from above.

But it is not so with the infant. The infant just learning to walk finds it a hard job, requiring all his concentration. This is the

prototype of all learning. When we learn anything, we build up a pattern of cooperation of nerve centers. In learning to walk, we build up a pattern of cooperation of centers innervating certain muscles, which must work together harmoniously in time and rhythm. Practice makes for skill and perfection. It develops "links" between nerve centers, so that a child of a certain age can walk without having to think of it.

The same is true of speech. The more you use a word, the better you know it. "Links" are formed between motor centers for the word and the neural patterns that underlie the corresponding images. Thus we learn that an object of a given size, shape, color and aroma is called an orange.

Automatic behavior is by no means limited to that which is mediated by the spinal cord. An automobile driver can drive skillfully while his mind is far away. Complex verbal behavior can be automatic too, as in the example of a friend of mine who was a chess expert. He was sitting in his living room playing a game while memorizing it, a task requiring intense concentration. During the game a friend named Bill walked in. The player looked up from the board and said, absentmindedly, "Hello, Bill." Ten minutes later, when the game was over, he relaxed, looked around, saw Bill and exclaimed in surprise, "Why, *Bill*, when did *you* get here?"

Disease may impair the "links" between nerve centers in lower levels, depriving these centers of the capacity to act automatically. The classical example is tabes. The tabetic is not paralysed, but he can no longer walk automatically, since the pathways for automatic locomotion have been disrupted. Proprioceptive pathways no longer send appropriate messages to motor centers in the cord. In order to walk the patient must dismiss everything else from his mind and must attend to his feet, concentrating on their position and on his next step. In a burning building he would be helpless, for excitement would interfere with the required deliberation.

In this respect tabes is the opposite of hysterical astasia-abasia. The hysteric has an intact mechanism for automatic locomotion. He cannot use it voluntarily, but in a situation of imperative need, as when the house is on fire, a patient who has not walked for years may run to safety without let or hindrance.

IMAGINATION; HALLUCINATION

The relation of brain levels to one another concerns a phenomenon that is much more important clinically than the inability to read without moving the lips; it concerns hallucination (5).

Hallucination is tied in with the function of imagination, but in a way that is often misunderstood. The psychotic patient is said to hear "imaginary" voices, and people are inclined to think his faculty of imagination is overactive. The opposite is in fact the case.

Light is shed on this problem by the mentation of the child. Here too there is misunderstanding, for there is a common belief that children have a "rich imagination," a misconception based on the fact, for example, that a small boy will float a block of wood in a puddle and pretend that it is a boat. But imagination is a complex function. It refers to the ability to see without the eyes, to picture an event in advance, to foresee a consequence. It would be preposterous to suppose that the child's power of imagination exceeds the adult's.

That children on the contrary have a *weak* imagination is proved by the following evidence, familiar to every parent. The child has a favorite story which he loves to have read to him. He can hear it a dozen times a day without tiring of it. He has heard it so many times that he knows it by heart, and if you should make the experiment of omitting a word, or if in a series of nouns or adjectives you should transpose the order of the words, he will sharply correct you. The

question then is: since he knows the story by heart, word for word, why does he beg you to read it to him? The answer is that he cannot enjoy the story in silence. He cannot *think* it unless he *hears* it. If no one is available to read it to him, he will recite it out loud, thereby gratifying his need to hear it. By contrast, the adult, with his greater powers of imagination, can think something even when he does not hear it. He can visualize without having to see.

In the hallucination of the psychotic adult we see the same principle at work. The patient *hears* what, if he were well, he would merely *think* (5). Far from having an overactive imagination, he is deficient in that faculty.

This fact explains a phenomenon seen in recovery from toxic delirium (6, 7). In the development of delirium, mental functions are disturbed in order of complexity: the most complex are the first to be disturbed. Correspondingly, in recovery, they are likely to be the last to be restored. To be sure, in some cases the patient makes a sharp and complete recovery, but there are others in which the two symptoms, disorientation and hallucinosis, clear up in a certain order. Specifically, disorientation clears up first, while hallucinosis lingers on for some days or weeks. Why does disorientation clear up *before* hallucinosis rather than after? There can be only one answer. Orientation is a relatively simple function, while the ability to think abstractly, to think without hallucinating, is more complex. We see this in the three year old child, who is oriented—he can answer such questions as "Whose house is this? Who is this man?"—at an age when he still begs you to read him his favorite story though he knows every word of it by heart.

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FACTORS IN MENTAL ILLNESS AMONG HOSPITALIZED CLERGY[1]

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INTRODUCTION

One hundred Catholic Priests, consecutively discharged from the acute male service of a private psychiatric hospital conducted under Catholic auspices, were the principal subjects for this investigation. These one hundred cases were collected over a seven year period, from 1952 to 1959.

The goal of this investigation was to attempt to gain as complete a picture as possible of those Catholic clergy who become psychiatrically ill, and a better understanding of their illness and of their hospitalization. All available pertinent information was gathered from the case histories of these subjects. This included data regarding their family background, educational, health and work histories, and their psychiatric history.

REVIEW OF LITERATURE

The only extensive study of mental illness among clergymen was reported by Moore (14). His study was essentially a comparison of the incidence of mental illness among Catholic priests and religious in the United States with the incidence of mental illness in the general population in the United States. Moore found a lower rate of mental illness among the religious group. His data show the comparative incidence of various diagnoses for diocesan priests and for priests of religious orders. Kelly (10) has more recently conducted a similar study, limited to religious Sisters in the United States. Her work is in general agreement with that of Moore's.

Neither of the above authors has made an attempt to analyze the religious groups they have studied in order to acquire a better understanding of the factors involved in their illness. Jahrreiss (8) made an effort to go beyond statistical data in an article which included limited case material on religious. He wrote, "But the many possible influences, wholesome and harmful ones, are so entwined in the life of priests and nuns that real insight into the actions and counteractions of these factors cannot be gained by statistics alone."

Moore (14), in the second part of his article, recognized the need for earlier detection of mental illness among prospective candidates for the religious life. "In general it is inadvisable to admit such persons (*i.e.* those who have suffered a mental break) to the priesthood or to the religious life." The increasing use of psychological testing and psychiatric interviewing of candidates for the religious life (2, 15) indicates a growing awareness among religious superiors that a vocation does not depend entirely on spiritual, intellectual and physical aptitude. The emotional component is gaining greater recognition as an important factor.

Demographic, environmental, and interpersonal correlates of mental illness have been studied effectively by many researchers. Scott (18) has summarized much of this work. The possible use of such data in better understanding and better evaluating clergy patients as well as clergy candidates is suggested by such work as that of Scherer and Nidorf (17), who were interested in familial relationships and environmental stress as prognostic signs. They found that psychiatric illness had a better prognosis in the absence of familial pathology.

No work of the kind to be reported here has been found in the professional literature.

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The individual variables used in this investigation have of course been studied in relation to other types of subjects. For example, Kohn and Clausen (11), in their article on the effect of parental interaction on schizophrenics, found that the mother held the dominant role in family decisions. Gregory (5) pointed out that it might be useful to investigate familial data such as parental age, family size and birth order in psychiatric patients. Hollingshead and Redlich (6), in their publication on the relationship of socioeconomic factors in mental illness, stated that upper and middle-class patients more frequently receive the benefits of psychotherapy and thus have shorter psychiatric illnesses. Jaco (7) brought out the fact that schizophrenics are more socially isolated than are manic-depressives. Tietze *et al.* (20), in their discussion of geographic mobility and mental illness, concluded that the incidence of emotional disturbance is greater among those who move from city to city. (This conclusion may have some application to the clergy patients presented here.) Frumkin (4), in his study of the relationship between kinds of mental disorders and socioeconomic level, suggested that the illness of lower status groups is directed against society, and that of higher status groups is directed against the self.

Wahl (22) reported a study of various familial and environmental factors in alcoholics. Martensen-Larsen (13) reported on the family constellation of homosexuals. These are only a few of the many publications on various factors related to mental illness in particular groups. The plan of this project was to isolate as many factors as possible from the case histories of the clergy patients and study the relationship of these factors to the mental illness of this selected group.

METHOD

Five case histories of clergy patients were carefully evaluated for factors that might be of importance or of interest in better understanding this group. From this initial pilot study, a working table of 72 variables was made. The 100 histories were then scrutinized most carefully, and the resulting data were collated and tabulated. It became obvious that many of the original 72 variables were not adequately represented in a sufficient number of the histories or were represented in such an ambiguous manner that their use involved subjective decisions of the investigators. For such reasons only 57 of the variables proved of continued value and were used in further analysis of data.

Both investigators worked on the case histories, dividing them equally and at random, discussing any question of interpretation of information. It seems safe to assume that any major differences of interpretation were thereby avoided.

It was later decided to obtain information on two other groups for partial comparison. The first group comprised 100 male patients consecutively discharged from the acute service of the same psychiatric hospital. Forty-two of these patients were Catholic, 58 were non-Catholic. This group was selected to include patients 26 years old and over, because the youngest member of the clergy group was 26. Seminarians and religious brothers were eliminated. All case histories of this group were studied by the same investigator. These 100 cases were collected over a two year period, from 1957 to 1959.

The second group for comparison comprised 100 non-hospitalized seminarians, all of whom expected to be ordained priests within four months of the time when they answered a questionnaire used to obtain the information to be reported here. The clergy group was composed of 57 diocesan and 43 religious priests; the seminary group was equally divided between 50 students for the diocesan clergy and 50 students for a religious order.

It should be clearly stated that neither the lay group nor the seminary group can qualify as strictly comparable to the clergy group with respect to all variables under

study. The original intent of this project was an intra-group study of the clergy patients; the other two groups were added as bases for comparison only in regard to some of the variables under consideration.

RESULTS

Table 1 presents the status of the two hospitalized groups as to medical or voluntary commitment. Voluntary admissions are seen to be more frequent among the clergy than among the lay group. The fact that clergy are under obedience makes it possible for them to be sent to the hospital with the instruction from their superiors to "stay there until the doctor discharges you." Even in their illness they continue to respect their obligation of obedience. As a result only 16 of the 100 had to be medically committed.

Table 2, giving the familial socioeconomic level of subjects in the three groups, indicates a significant difference in socioeconomic background between clergy and lay patients. There is a greater number of clergy with lower socioeconomic backgrounds than occurs among lay patients. One obvious factor involved in this difference is the expense of a private as opposed to a state hospital. Laymen, who must usually pay for their own psychiatric treatment, would go to a state hospital much more quickly than the clergy, whose hospitalization is paid for by their diocese or religious order. The seminary group showed a significantly greater number of middle income backgrounds than did the clergy patient group. This may represent a national economic trend to an expanding middle class society; or it may represent a specific difference between the two clerical groups, suggesting the possibility that the clergy from lower socioeconomic strata have a more difficult time adjusting to a vocation which places them in the middle or upper class of our society.

TABLE 1
Commitment Status

	Voluntary	Committed
Clergy (N = 100)	84	16
Lay (N = 100)	64	36

S.E. D% = 6.01
C.R. = 3.33 (.01 level)

TABLE 2
Socioeconomic Level

	Lower	Middle	Upper
Clergy (N = 68)	74%	23%	3%
Laity (N = 84)	44%	45%	11%
Seminary (N = 100)	22%	66%	12%

Chi-square (clergy-lay) = 14.58 (.01 level)
Chi-square (clergy-seminary) = 84.06 (.01 level)

TABLE 3
Scholastic Achievement

	A student	B student	C student
Clergy (N = 68)	48%	16%	36%
Laity (N = 63)	13%	30%	57%
Seminary (N = 100)	21%	71%	8%

Chi-square (clergy-lay) = 52.36 (.01 level)
Chi-square (clergy-seminary) = 80.76 (.01 level)

Table 3 shows the reported scholastic level of the subjects in the three groups of patients. There is, as might be expected, a preponderance of clergy who were "A" students as compared to either the seminary or lay groups.

Subsequent data were arranged according to three main areas of interest insofar as applicable to each of the three groups. The kind of information that was tabulated and evaluated is indicated by the following outline.

A. Family background
   1. Dominant parent
   2. Proximate parent
   3. (a) Psychiatric symptoms in parents
      (b) Alcoholism in father
   4. (a) Family constellation[4]
      (b) Family size[4]

[4] Data were judged not significant, and were eliminated from the following tables. Available on request from authors.

B. Psychiatric history
   1. Problems in seminary
   2. Previous psychiatric treatment[4]
   3. Age of onset of major presenting psychiatric symptom
   4. Age of onset of major presenting psychiatric symptom in relation to time of ordination
   5. Duration of major presenting psychiatric symptom[4]
   6. Age at time of this hospitalization[4]

C. Present hospitalization
   1. Patient's reason for coming to hospital
   2. Duration of hospital course
   3. Improvement in hospital
   4. Disposition on discharge
   5. Diagnosis on discharge

The clergy and the lay groups were subdivided according to the psychiatric symptom or group of symptoms which precipitated their hospitalization. Such a subdivision seemed more valid and useful than one according to diagnoses. This procedure, at least, partially avoided the potential unreliability of diagnostic categories (16). The patients seemed to fall quite naturally into nine groups, which are listed here.

1. Alcohol—patients whose use of alcohol obviously deviated from chosen goals and created problems for them.

2. Anxiety—patients who exhibited one or several of the following symptoms:

restlessness	agitation
nervousness	tension
uneasy with others	insomnia
crying spells	unrelated fear

3. Delusions and hallucinations—patients who characteristically had no complaints but for whom these symptoms had created problems.

4. Depression—patients who exhibited one or several of the following symptoms:

feeling of inertia	unusual job failure
loss of interest	severe fatigue
excessive worrying	exaggerated somatic complaints

5. Drugs—patients whose use of drugs obviously deviated from chosen goals and created problems for them.

6. Inadequacy—patients who exhibited one or several of the following symptoms:

feeling unstable	excessive dependency
feeling insecure	lack of personality
feeling left out	severe authority problems

7. Obsessive-compulsive—patients who exhibited one or several of the following symptoms:

morbid scruples	severe indecisiveness
obsessive thinking	compulsive behavior

8. Senility—patients who exhibited the confusion, memory loss and disorientation of chronic brain disease.

9. Sex—patients whose sexual behavior obviously deviated from chosen goals and created problems for them.

TABLE 4
Major Psychiatric Symptoms

	Clergy	Lay	Total
Alcohol	32	10	42
Anxiety	30	21	51
Delusions-Hallucinations	7	21	28
Depression	7	36	43
Drugs	3	1	4
Inadequacy	6	3	9
Obsessive-Compulsive	4	3	7
Senility	1	3	4
Sex	10	2	12

Rho = .49 NSD

Table 4 shows the distribution of clergy and lay patients according to the nine categories defined above[5]. There is no significant difference between the two groups.

Table 5 compares the three groups with respect to the parent described as "dominant" in the family and the parent described as "closer" to the subject. The mother was reported to be the dominant figure in the home of 91 per cent of the clergy patients. This is a significantly higher figure than in either the lay or the seminary group. This maternal dominance may create some dis-

[5] Information in all subsequent tables has been compiled according to these nine subdivisions and is available from the authors.

tortion in the priestly role or in the relationship to authority figures, and act as a factor in psychiatric illness among the clergy. Bieri and Lobeck (1) have pointed out a background of maternal dominance in men who have difficulty in accepting authority.

Table 6 indicates the number of subjects who reported psychiatric illness in parents, based on such criteria as suicide, alcoholism, psychiatric treatment, extreme "nervousness," etc. Data on clergy and lay groups are included. It is seen that a significantly greater number of clergy patients had parents with psychiatric symptoms than did the lay patient group. The clergy more frequently had alcoholic fathers than did the lay or the seminary group. Eleven of the 32 clergy with alcohol problems had alcoholic fathers. These data—and that of Table 5 as well—suggest that the psychological importance and the psychic impact of parental figures is greater among the clergy than among a comparable non-clergy patient group. Moore and Sullivan might say that the parataxic distortion created by parental figures remains more effective in the clergy group. This seems a reasonable conclusion, because these early meaningful relationships are not as easily displaced or modified as in the non-clergy group. Laymen have new, deep and close interpersonal relationships with associates in love affairs and courtship, in marriage, and with their own children. These interpersonal experiences tend to modify and frequently to mollify their earlier experiences with parents so that the effect of parental influence is diminished. Walters (23) found a high percentage of alcoholic fathers in the group of alcoholics he studied; even more important, he found the inclination to drink more precocious in those from very religious homes.

TABLE 5

Parental Influence

	Father Dominant	Mother Dominant		Father Closer	Mother Closer
Clergy (N = 43)	9%	91%	(N = 49)	12%	88%
Lay (N = 49)	31%	69%	(N = 54)	24%	76%
Sem. (N = 76)	43%	57%	(N = 76)	25%	75%

	Dominant Parent	Closer Parent
Clergy-Lay	S.E. D% = 7.93 C.R. = 2.78 (.01 level)	S.E. D% = 7.45 C.R. = 1.62
Clergy-Seminary	S.E. D% = 7.11 C.R. = 4.79 (.01 level)	S.E. D% = 6.82 C.R. = 1.91

TABLE 6

Psychiatric Symptoms in Parents

	Symptoms Present	Symptoms Absent
Clergy (N = 42)	86%	14%
Lay (N = 78)	38%	62%

S.E. D% = 7.71
C.R. = 6.23 (.01 level)

Table 7 presents the number of clergy who experienced serious emotional problems in the seminary. These problems included severe and continued doubts about their vocation, advice from authorities to leave, sexual maladjustments, gross problems with authority, morbid scruples, severe anxiety attacks, and acute emotional illnesses. Only 40 histories provided sufficient information to allow a rating; of these, 31 or 77 percent gave a positive response. The table also reports the major presenting psychiatric symptoms. The high incidence of positive responses suggests a greater need for more careful screening of candidates for the clergy, as is now being done in many areas (15), and for more careful attention to the emotional growth and health of seminarians on the part of seminary faculties.

The ages of onset of major presenting psychiatric symptoms is significantly different for the clergy and lay groups, as shown in Table 8. The clergy group is seen to have had an earlier onset. The greater stress of seminary life at an early age and the stress of clerical responsibilities when still quite young may be factors in accounting for this difference. Another factor to be considered

is the difference in diagnoses between the two groups. There were 25 affective and involutional psychoses among the lay group as opposed to seven among the clergy, and these illnesses typically have a late onset.

The time of onset of major presenting psychiatric symptoms in relation to time of ordination, is presented in Table 9. Here one may note that none of the alcoholic problems were reported present prior to ordination. In 67 alcoholics studied by Williams (24), 64 began drinking before the age of 25. Certainly the rather sheltered seminary life would tend to keep any such tendency in check until after ordination. The onset of symptoms has no apparent relationship to time of ordination. Problems in the sexual sphere were present in nine of 10 cases prior to ordination, suggesting that neither the vow of celibacy nor the priestly function created this psychic conflict. It was present prior to ordination and probably represents a psycho-sexual fixation rather than a regressive phenomenon. Woodward (25) reported on 113 cases of homosexuality, of which 38 per cent had begun before the age of 15.

One of the important problems of treatment of clergy patients is indicated in Table 10. Seventy-two of the lay patients came to the hospital for the express purpose of getting psychiatric treatment, whereas only 15 of the clergy reported this motive. Seventy-seven of the clergy were in the hospital either for symptomatic relief or because they felt that they were forced to come by their superiors. The problems which such attitudes, and particularly the latter, may create for therapy are obvious. These attitudes may be a reflection of a more general feeling among some clergymen who regard psychiatry with suspicion and doubt. The type of preparation for hospitalization is also an important factor. A referring physician usually tries to help the patient approach this situation as beneficial. In many clergy referrals, although a physician arranges the hospitalization, the interpretation of it to the patient comes directly from religious superiors, who may already have lost considerable objectivity in the case and be quite personally annoyed by the symptomatic behavior of the patient. As a result the procedure may be perceived as essentially punitive.

Further evidence of the poor therapeutic

TABLE 7

Emotional Problems in Seminary

	Number Responding	Number with Problems	% Positive Responses
Alcohol	7	5	71%
Anxiety	12	9	75%
Sex	9	8	89%
Other	12	9	75%
Total	40	31	77%

TABLE 8

Onset of Psychiatric Symptoms

	Before Age 20	Age 20–32	Age 32–52	Over Age 52
Clergy (N = 97)	19	42	34	2
Lay (N = 100)	7	35	40	18

Chi-square = 11.76 (.01 level)

TABLE 9

Symptoms Related to Ordination Time

	Prior to ordination	0–5 yrs. after ordin.	6–10 yrs. after ordin.	11–20 yrs. after ordin.	Over 20 yrs.
Alcohol	0	19	8	3	2
Anxiety	14	7	4	5	0
Sex	9	1	0	0	0
Other	13	7	3	2	3
Total	36	34	15	10	5

TABLE 10

Acceptance of Hospitalization

	Treatment	Symptom Relief	Forced	Evaluation
Clergy (N = 100)	15	43	34	8
Lay (N = 100)	72	12	14	2

Chi-square = 100.45 (.01 level)

motivation of the clergy is suggested in Table 11, which presents duration data. The duration of hospitalization for the clergy is significantly greater than it is for the lay group. Several factors besides poor motivation may be involved. The clergy patients, for example, do not have to meet the economic demands of their own hospitalization. It is also possible that the staff physicians may — consciously or unconsciously — be more exacting in recommending discharge for their clergy patients, as a function of the doctors' feelings about the importance and the obligation of the priestly office. The greater proportion of personality disorders among the clergy, as shown in Table 14, would also lengthen the period of their hospitalization.

Table 12 indicates that a significantly greater number of lay patients were discharged as improved than were the clergy. The observations made above may again apply here. Poorer motivation, greater number of personality disorders, higher standards by the hospital staff in evaluating the clergy, may all be factors contributing to this difference.

Table 13 reports the disposition of patients at time of discharge. A greater number of lay patients returned to the same job than did clergy patients, a third of whom obtained a change of duty. This may reflect a greater facility and flexibility in assigning clergy. Laymen do not so easily move from job to job or town to town. Another possible influence is that superiors may feel that a priest's status is somewhat jeopardized by his illness; therefore they prefer to transfer him to another parish or another duty. The numbers returning to some gainful duty are approximately the same for the two groups.

TABLE 11
Duration of Hospitalization

	Less than 1 month	1–6 months	7–18 months	Over 18 months
Clergy (N = 100)	19	48	22	11
Lay (N = 100)	43	51	6	0

Chi-square = 64.56 (.01 level)

TABLE 12
Improvement at Discharge

	Improved	Unimproved
Clergy (N = 100)	61	39
Lay (N = 100)	81	19

S.E. D% = 6.21
C.R. = 3.22 (.01 level)

TABLE 13
Disposition at Discharge

	Same Duty	Change of Duty	Transfer to other hosp.	Died
Clergy (N = 77)	47%	34%	18%	1%
Lay (N = 75)	76%	11%	13%	0%

Chi-square = 28.51 (.01 level)

The data of the last several tables seem to point to one salient fact. Whatever factors may be involved in the difference between the clergy and lay groups, it is clear that clergy patients have more difficulty with psychiatric hospitalization than do lay patients. The clergy have more negative attitudes about their hospitalization; they require longer periods of confinement; they show less improvement at time of discharge; and they are less able to return to their previous duties.

Table 14 reveals a significant difference between the discharge diagnoses of the clergy and the lay groups. There is a greater number of psychotic and psychoneurotic depressive reactions among the lay group, and a greater number of sociopathic personality disturbances among the clergy group. The much greater number of depressive reactions among the lay group lends itself to some speculation. Many of these depressions in lay patients were precipitated by a loss of employment (or threat of such loss), or by financial or business failure (or threat of such failure). Evidence in the study of Malzberg (12), supports this observation. Such dire events are not a vocational hazard

TABLE 14
Discharge Diagnoses

	Organic brain synd.	Psychoses	Psychoneuroses	Personality disorder
Clergy (N = 99)	6	26	21	46
Lay (N = 100)	7	40	27	26

Chi-square = 21.89 (.01 level)

to the Catholic clergyman. He may face retirement but usually is not forced into it, as many lay people are. The clergyman normally does not lose the security of his fundamental position. He may fail at a post and be removed, but he continues to function as a priest. Whether or not his spiritual orientation and values combine to an asset in warding off depression is moot. The relationship of religion to psychodynamics has been studied by Eichler and Lertzman (3).

The 15 sociopaths among the clergy group are rather striking. Seminary training and the clerical life lend themselves easily to a lack of duration and depth in interpersonal relationships. For many the "spirit of detachment" becomes synonymous with "fugitive, fleeting, involvement with other people," words which Sullivan used so aptly to describe the sociopath. Sociopaths are perhaps attracted to the challenge of the clerical life, since they need to prove themselves. They are perhaps more comfortable in the impersonal relationships of seminary life, in their need to keep a distance between themselves and others. Forty-six of the clergy were diagnosed as personality disorders, suggesting the presence of life-long patterns of maladjustment. These patterns of maladjustment must, therefore, in most cases have preceded the clerical state.

SUMMARY

Case histories of one hundred Catholic priests consecutively discharged from a private psychiatric hospital have been studied in detail for factors common to this particular group and for factors differentiating sub-groups. Partial comparisons were made with a group of one hundred male patients discharged from the same hospital and one hundred Catholic seminarians approaching ordination.

Diagnoses, major symptomatology, and age of onset of illness differed considerably between the two patient groups, as did the socioeconomic background, school achievement, and parental influence. The clergy patients had notably greater difficulty accepting hospitalization and treatment than did the lay group. A large percentage of the clergy reported psychiatric problems prior to ordination.

Limitations of space have not allowed the presentation of a considerable amount of additional information on the clergy group: these data will be examined in a later publication.

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BRIEF COMMUNICATION

SERUM PROTEIN ABNORMALITIES IN PSYCHOSIS: A PRELIMINARY REPORT[1]

W. J. FESSEL, M.R.C.P.[2]

As part of a wider program to investigate abnormal serological reactions shown by patients with various psychoses, we have performed FII latex and nucleoprotein latex agglutination tests on serum from psychotic patients in Napa State Hospital. These tests are used clinically in the diagnosis of rheumatoid arthritis and systemic lupus erythematosus respectively. They depend upon the presence of an abnormal globulin in the serum in those diseases.

Sera from psychotic patients were drawn under aseptic conditions and stored at −15° C. Control sera (blood bank donors including 72 persons in Alcatraz Federal Prison) were obtained from the Irwin Memorial Blood Bank. A slide test[3] was used to screen the sera for their ability to agglutinate gamma globulin coated latex particles; sera showing a suspicious degree of agglutination in that test were subjected to the standard tube dilution FII latex agglutination test (6). Agglutination was graded ± to ++++. Only agglutination of grade + or more in a titer of over 1:20 was considered positive; however, we have noted that many sera from psychotic patients persistently caused ± agglutination in several tubes whereas this was never found in sera from normal persons. Of 524 sera from psychotic patients, 40 (7.6 per cent) gave positive FII titers ranging from 1:40 to 1:5620. Most titers were in the range of 1:320 to 1:1280. Of 317 control sera only 4 (1.3 per cent) were positive. The difference in positive tests shown by patients and controls is statistically significant ($p < 0.05$)[4]. Five of the 40 patients with positive FII tests have clinical evidence of connective tissue disease (probable or possible rheumatoid arthritis (3)[5], Marfan's Syndrome (1) and rheumatic heart disease (1)).

Filter paper electrophoresis of the 40 FII positive sera showed abnormal globulin patterns in 37. These abnormalities were various. There were found, in a number of sera, abnormalities in the alpha globulins, splitting of the beta globulin band and a definite hypergammaglobulinemia. These abnormalities were, however, also present in the sera of psychotic patients having negative latex agglutination tests. They were not present in the normal sera investigated by filter paper electrophoresis. These surprising findings are being further investigated and extended.

To exclude liver damage as a cause of the abnormal serum protein, we made serum glutamic oxalacetic transaminase estimations[6] on the FII positive sera. The serum transaminase level is a sensitive measure of liver cell injury, and is not positive in rheumatoid arthritis or systemic lupus (7). Although six of our patients had a history of alcoholism only three sera showed elevations from the normal range and none of the three was from a patient who is a known alcoholic.

[1] This work is part of a project supported by the California State Department of Mental Hygiene. I am also indebted to Dr. Enoch Callaway, III, Director of Research at Langley Porter Neuropsychiatric Institute, for his encouragement and advice, and to John Koch, B.S., for technical help.

[2] The Langley Porter Neuropsychiatric Institute and the Department of Psychiatry, University of California Medical Center, San Francisco, California.

[3] Supplied by Hyland Laboratories, Los Angeles, California.

[4] I am indebted to Mr. S. Autumn for statistical calculations.

[5] American Rheumatism Association Criteria.

[6] Method of Sigma Chemical Company, St. Louis, Missouri.

Normal liver function tests do not eliminate the possibility of a damaged liver and further studies of this are being made. Unfortunately, serum transaminase levels may be raised in brain disease (3) and most other liver function tests may be strongly positive in rheumatoid arthritis and systemic lupus despite histologically normal liver (5).

We considered the possibility of an artifact resulting from long confinement and from the general living conditions in a mental hospital. This point has been stressed by Kety (4). We have tested the serum from 72 prisoners in Alcatraz Federal Prison: only one was FII positive. Also, none of the sera from 41 patients in hospitals for feebleminded were positive. These results would seem to argue against chronic confinement or chronic malnutrition as an explanation.

Nucleoprotein latex agglutination tests were made on 724 sera from psychotic patients by the method described in a prior article (1). Of these 724 sera there were 9 (1.2 per cent) with positive nucleoprotein latex agglutination tests compared with none of 175 sera from blood bank donors. Five of the nine patients with positive nucleoprotein latex agglutination tests have clinical evidence supporting a diagnosis of systemic lupus erythematosus or other connective tissue disease. There was no overlap between FII latex positive and nucleoprotein latex positive sera.

Thus, a small percentage of psychotic patients have clinical connective tissue disease, whereas a much larger number have serological evidence, as measured by FII latex and nucleoprotein latex tests, supporting that diagnosis. Although no estimate is available of the incidence of such disease in the general population, the mean incidence of psychosis in systemic lupus is as high as 22 per cent (2).

Since both systemic lupus erythematosus and rheumatoid arthritis are currently considered to be autoimmune diseases, it is possible that the psychosis in patients whose sera show positive FII or nucleoprotein latex agglutination tests is a result of an autoimmune response to a constituent of brain. To test this possibility we used a micromodification of the Ouchterlony technique to examine these sera for their ability to react with a calf brain extract. Only one of 310 sera (including those causing latex agglutination) from psychotic patients caused a precipitin reaction; whereas this reaction was produced by two of 41 sera from patients having either Schilder's disease or postencephalitic mental deterioration. Since the sera causing a precipitin reaction with the brain extract were FII negative it is unlikely that positive FII tests reflect an autoimmune response against a brain constituent, but we are repeating the tests using a human brain extract.

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BRIEF COMMUNICATION

A CONTRIBUTION TO THE ANALYSIS OF DÉJÀ VU

JEROME M. SCHNECK, M.D.[1]

Episodes of *déjà vu* are experienced by normal people and have been seen in psychoses, neuroses and epilepsy. At times, fatigue and intoxications may predispose to its occurrence. In general it has been accounted for on the basis of associations between current situations and past happenings. The forgotten elements are assumed, however, to involve psychological conflict and to have been repressed. Sometimes *déjà vu* is said to take place especially in young people with daydreaming tendencies (6).

PSYCHOANALYTIC LITERATURE

In 1913 Freud (3) published "Fausse Reconnaissance (Déjà Raconté) in Psycho-Analytic Treatment." In it he reviewed opinions about *déjà vu*. His article is readily available; details will not be repeated. It is pertinent to note that he mentioned his agreement with the view, offered by Grasset in 1904, that *déjà vu* implies an earlier unconscious perception making its way into consciousness as a result of a current, similar impression. He tells of this explanation made in 1907 as his own, without the knowledge of Grasset's earlier contribution (*La sensation du déjà vu*). In 1936 he mentioned *déjà vu* as a counterpart of depersonalization and derealization in that an attempt is made to accept something as belonging to the ego instead of furthering its exclusion (4). I wonder whether it would be more correct to say that *déjà vu* reflects only in part an attempt at acceptance, but more accurately the ambivalence associated with recognizing an area of conflict that lies at the core of the phenomenon.

Fenichel (2) says that repression has been completed in *déjà vu*, the ego does not want to be reminded of the repressed material, and the feeling of *déjà vu* is the result of being reminded contrary to its will. It is doubtful that this is sufficiently explicit, and an attitude of ambivalence seems to be the more likely ingredient.

Marcovitz (5) stressed the idea that *déjà vu* reflects the desire for a second chance. The wish implies "I will do better next time." He notes that Ferenczi linked *déjà vu* phenomena to recent or distant dreams, and feels that the relation to dreams or unconscious fantasies as mentioned by Freud and Ferenczi does not offer a specific characteristic such as he proposes. "The wish is for a repetition, for a second chance, so that the outcome may more closely correspond to a wish."

More recently, Arlow (1) supplied a case and discussed the phenomenon in detail, with many additional references to the literature. A variety of claims and speculations emerged. Perhaps there is a tendency to try to account for all such occurrences with one encompassing explanation instead of allowing for variations in psychodynamics despite some similarities.

CASE REPORT

A 38 year old homosexual artist had *déjà vu* experiences dating back to childhood. In his analysis he recalled the first at age eleven. He was very angry with his mother, extremely anxious, and felt he wanted to die. In a tantrum he destroyed some prized possessions and as an adult he interpreted this as a probable symbolic substitute for suicide. The *déjà vu* occurred then and intermittently thereafter.

The usual pattern of the *déjà vu* involved the patient in a social setting with one or more people in "intimate" conversation. An

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aura would come over him of having lived through this before. The special attribute in the feeling that he had previously heard what was being said and experienced what was then occurring lay in the impression of some connection with previous dreams. This did not imply for him at the time a link with a special dream or with dreams of specific content. The chief ingredient of the *déjà vu* was an element of anxiety and the uncomfortable feeling that "something awful was going to happen." This feeling related to the current setting and the vague idea that there was something similar in past dreams the content of which he could not remember.

In an attempt to understand some features of the *déjà vu* episodes, an examination of his dreams seemed enlightening. A frequent and disturbing type of dream would find the patient having to accomplish something, yet unable to do so. This followed the familiar pattern of wanting to get someplace and being unable, wishing to obtain something but failing, and similar problems. The theme seems to resemble common dreams of examination anxiety and the missing of trains. The patient's dreams entailed much feeling of frustration.

Of special note was dream content involving his wanting food yet being unable to obtain it. The patient complained of much gastrointestinal discomfort with nausea, retching, anorexia, and difficulty in retaining most foods, liquid and solid.

Apparently the dreams of greatest significance were of acutely delineated castration. The pattern involved a homosexual or heterosexual situation (the patient claimed to have had some heterosexual relationships). In the dreams he would wish strongly for erections of "immense proportions." His penis would increase in size. Then it would break, become completely detached from his body, leaving him mutilated. He would experience nightmarish anxiety.

DISCUSSION

Of these repetitive dream patterns it would seem that the last was basically involved in the underlying associative fantasy during *déjà vu* episodes. ("Something awful was going to happen.") The basic theme was castration anxiety which undoubtedly had a more general corrosive influence on him. The additional aforementioned dream patterns clearly reflected his problems too. He was aware of persistent unresolved hostility toward his mother. His gastrointestinal difficulties required hospitalization. His sexual problems were quite evident. He was alcoholic and had become habituated to paraldehyde. He was severely blocked in his creative efforts and plagued with chronic, pervasive anxiety.

The patient's castration fears were so intense as to break through the protective webs of dream distortion. Mechanisms of defense failed to ward off unconscious pressure erupting pointedly in the waking experiences of "intimate conversations" (the patient's description) with their sexual implications. The ingredients of these personal contacts triggered anxiety linked to the unconscious conflicts reflected in his dreams of castration. The *déjà vu* may be in part the dream substitute, accounting for the feeling of some connection with previous nocturnal dreams. He experienced anxiety and dread of what was going to happen. Indeed, in his dreams something terrible did happen.

The case data are consistent with the theme mentioned by Freud of an earlier perception making its way into consciousness. The presence of a current, similar impression in this instance is inferred. The suggestion of an attempt in *déjà vu* to accept something as belonging to the ego may fit in with the facts. More pertinent here may be the qualification stated earlier. There is a reflection of ambivalence associated with recognizing the area of conflict (surrounding the theme of castration in this case) that lies at the core of the phenomenon.

The unpleasant feeling in this patient's *déjà vu* episodes is consistent with Fenichel's idea that the ego does not want to be reminded of the repressed material. However,

repression is evidently not as complete as he implies nor does his view actually explain the phenomenon itself.

The implication of a second chance to achieve a better outcome is questionable in this case unless one believes it pertains to attempts to master the underlying anxiety. The latter view seems to hold true on occasions for an explanation of repetitive dreams. Indeed the castration dreams, as well as the others that have been mentioned here, were repetitive in this patient. Some may choose to see the repetition in the dreams and in the *déjà vu* episodes as fundamentally masochistic.

Regardless of differences of opinion about psychodynamics in general and the implications of *déjà vu*, the case presented here highlights the link not to one preceding dream, but to repetitive dreams of special content. The content involves severe castration anxiety clearly evident in these dreams and reflected in the patient's symptoms and general personality disturbance.

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PIAGET, JEAN, WITH INHELDER, BÄRBEL AND SZEMINSKA, ALINA. *The Child's Conception of Geometry.* Basic Books, New York, 1960. vii + 411 pp. $7.50

Jean Piaget and his co-workers have given to the field of child psychology some twenty-four separate volumes—an unofficial count—dealing with almost as many different aspects of cognitive development. These include works on the development of moral judgment; play, dreams, and imitation; intelligence; language and thought; and on children's conceptions of the world, reality, space, quantity, physical causality, time, movement and logical operations. These volumes constitute a veritable encyclopedia of intellectual growth, an undertaking of truly heroic proportions.

The present volume, dealing with the development of "Euclidean thought" is not a new work. It was first published twelve years ago in Paris by the Bibliothèque de Philosophie Contemporaine, Presses Universitaires de France, under the title *La Géométrie Spontanée de L'enfant.* (Basic Books does not mention the earlier publication.) A few up-dated references in footnote form have been added to this translation.

The organization of the book reflects the senior author's considerable experience in planning a comprehensive program for normative study and in reporting on it. Each chapter represents a separate investigation, presenting, first, a theoretical analysis of the problem, *e.g.*, subdividing a straight line; second, a description of one or more empirical demonstrations; next, examples of children's responses; and finally, a discussion of the demonstration apropos Piaget's theoretical position. The separate chapters are arranged according to developmental level and are grouped into three sections. Integration and transition for the sections are provided by an introductory and a concluding chapter.

So much for matters of form and style. What about the theory and demonstrations themselves? Piaget's approach to both is different from most current American approaches to psychological research, and these differences warrant some examination and comment.

Piaget's descriptive classification of cognitive development or, as he calls it, the "theory of genetic epistemology," has been presented on a more abstract and inclusive scale in other publications. (See, for example, *Introduction à L'épistèmologie Génétique.* Presses Universitaires de France, Paris, 1950.) In the present work, it is naturally limited to "metrical" aspects of cognition. But in both the general and particular cases, the theory of genetic epistemology is outstanding in two respects, both having to do with its derivation. First, it is a theory that to all appearances has not been influenced in any respect by either the theories or research of other developmental psychologists. Against a background of other psychological theories, it stands out as an excellent example of originality—or isolation, according to one's point of view on such matters. This will be mentioned again later in its reciprocal form, the influence of Piaget's genetic epistemology on the theories of other workers.

A second unusual aspect in its ancestry is that this theory is a logically derived one, not the conceptual distillation of large quantities of research data (either Piaget's or anyone else's). Not even the wild flowers of clinical evidence intrude into this cerebral terrain. The theory is all logic and reason. Some idea of this can be had from an example, as in this theoretical statement regarding the child's concept of position: "To measure is to take out of a whole one element, taken as a unit, and to transpose this unit on the remainder of a whole; measurement is therefore a synthesis of sub-division and change of position" (p. 3).

What kind of methodology accompanies the theoretical presentation? It consists of the same sorts of ingeniously devised, sophisticatedly simple demonstrations that have been a Piaget trade-mark for the last 30 years. To illustrate, here is the description of the basic task from the chapter "Subdividing a straight line."

> Two straight lines A_1 C_1 and A_2 C_2 (or A_2 D_2 if the second line is longer than the first) are represented by lengths of string attached to nails. A bead is threaded to each and placed at A_1 and A_2, that is, at the beginning of both lines. The child is told: "The bead is a tram travelling along its track. My tram is going this far (moving the first bead from A_1 to B_1). I want you to make your bead do a journey which is just as long as mine, one the same length, etc. Now you see I'm doing this journey (A_1 B_1), how far will have I to go on your track (A_2 C_2) to do a journey the same length?" (p. 129)

The examiner then asks other questions, each of which is contingent upon the child's preceding response.

As the senior author himself has described his method, it lies somewhere between observation on the one hand, and standardized procedures of test administration on the other. His term for it is the "method of clinical examination"; it is the same method, he has explained, as psychiatrists use in making a diagnosis. It is, in essence, a partially structured play interview with a slightly standardized administration. (An idea of the "looseness" of standardization can be had by turning to the response examples for any one demonstration and reading only the examiner's statements.) When used in the laboratory rather than for diagnosis, the method of clinical examination turns empirical work into what is more accurately described, on a generic level, as a "demonstration" than it is as "research," a term that has been limited traditionally to use of the scientific method.

The relation *between* theory and demonstration is another unusual facet of these investigations. Earlier, the point was made that Piaget's theory has remained uninfluenced by other psychologists' research. It seems equally true, though less obvious, that Piaget's theory has likewise been uninfluenced by his own empirical work. The role assigned to demonstrations in Piaget's theoretical world is reminiscent of the Georg Kaiser lines, "I am a poor relative and have no life of my own," for every demonstration is relegated to the same position of minor importance and maximum predictability. The only real data that the demonstration is allowed to contribute are the number of developmental levels—the developmental levels themselves, but not their number, having been specified by the theory. Otherwise, each demonstration functions to illustrate, to humanize, to persuade on behalf of the theory. But it does not test any part of the theory. It is kept at a respectful distance from the theory, allowing it to re-emerge inviolate and unprofaned by any need for experimentally-imposed modification. In this way, each chapter ends essentially as it began: the opening theoretical exposition is restated at the end, its content unchanged except for the age-level specifications.

What is the importance of *The Child's Conception of Geometry*? First, what does it yield in the way of current potential practical applications? And second, what does it contribute to other theories?

The authors take up the first issue in the second paragraph of their preface. They write that their volume will be of interest to educators, and end the paragraph by stating that, "It is not difficult to see how the facts brought to light in the present study of the psychogenesis of metrical notions may some day be given a practical application in teaching" (p. vii). This first reference to practical applicability is also the last in the book. The teacher is on his own for all 408 pages of text.

Unfortunately, the authors' lighthearted confidence in the ability of their readers, teachers or otherwise, to bridge the long gap between laboratory and life, is hardly warranted. It *is* difficult to see how these facts may be given a practical application in

teaching; in fact, it is impossible. In the first place, the geometrical content of these investigations is quite outside the scope of the elementary education curriculum—at least American elementary education. First grade teachers are *not* faced with the problem of teaching six-year-olds how to measure an angle or how to double an area or how to determine a point in either two or three-dimensional space. And how many curriculum supervisors will ever seriously consider adding geometry at any elementary school level? There are simply too many things for young children to learn that are held by educators to be more important than geometry.

The second reason that *The Child's Conception of Geometry* yields nothing of practical value for teachers is that Piaget's method of clinical examination bears no relation whatsoever to teaching methods. Teachers do not interview students, they teach them; and if the research worker really wishes to generalize to the training process of the classroom, he must use the appropriate methodological counterpart in his laboratory—the experimental method.

These comments are meant only as a denial that the book has any practical value, and not as a condemnation of the authors for failing to provide it (except for the minor point that it isn't really cricket to make airy claims for applicability without documenting them). Certainly there is nothing to compel the academically employed psychologist to manufacture salable research products, and in fact, there are all sorts of cogent reasons that psychologists can muster in support of a research policy of ignoring practical issues. Still, there is this to consider: there is not much point in doing research at all unless it promises to achieve some value in some respect—by contributing to theory, to general experimental methodology, or to practice. To shoot for all three goals is the most obvious way of maximizing potential value—or, to think of it in another way, of minimizing the probability that one's research may be consigned to oblivion.

Turning to the second question, what contributions do these investigations make to other theories? The answer to this goes back to earlier comments about the unique life history of genetic epistemology. Other theories are born, make a public debut, and promptly lose their virginity; they absorb criticism from other theorists; they change as experimental evidence demands; and they lend a few original or demonstrably valid points to other theories. But Piaget does not borrow from others, nor does he try to sell his own theory. Just as in the matter of practical application, he leaves this to the reader. Perhaps it is partly for this reason that other developmental theories seem not to have been influenced by Piaget's. The theory of genetic epistemology appears neither to contradict nor support others, but rather to stand apart—the nature of its relation with the family of developmental theories ambiguous and unclear.

Returning to the empirical part of the investigations, there is still one important matter for comment, and that concerns the authors' failure to describe their work fully, to provide essential information about subjects, procedures, and results. The matter of basic requirements in reporting research is not often a source of great concern in the United States, where most psychological research is published in journals; if the author himself leaves some descriptive gaps in the "Method" section of his article, they are generally called to his attention by the journal editor—as a condition of publication. These publication requirements are not maintained because of musty tradition, but in order to safeguard scientific objectivity, to ensure replicability, and simply in order that other scientists may judge for themselves the value they are willing to place on the study, *i.e.*, the degree of generalization it merits, and the degree to which the author's conclusions are, indeed, supported by the adequacy of his sampling, his design, his procedure and his analysis of results.

Too many essential items of information are omitted from *The Child's Conception of Geometry*. Here are some of them.

Subjects: What number of subjects was used in each demonstration? Their ages? Sex? What was their standing in regard to those variables having a high probability of affecting the results? For example, intelligence? Academic background and achievement? Previous laboratory experience? *I.e.*, were the same subjects used in more than one demonstration?

Procedure: What were the criteria by which subjects' responses were categorized as belonging to one developmental stage rather than to another? Was any attempt made to ensure, or even to assess, the reliability of this categorization? For example, were there at least two judges? Did they work independently, if not disinterestedly? To what extent did their judgments agree?

Results: The question here is simply and all-inclusively, what *were* the results? There are no inferential statistics; there are no descriptive statistics; worst of all, even the data are missing. There are case study illustrations, but in a presumably scientific work, these are not allowable replacements for a general statement of results. Likewise, the words "younger" and "older" are quite inadequate substitutes for numerical ages, particularly when age level is the one and only concern of this study.

What do the authors conclude from their unpublished results? Here, without further comment, is their own summary.

> The whole of this volume has been concerned with Euclidean notions and with measurement. . . . We may distinguish three levels of achievement in the construction of Euclidean space. The first level is represented by the qualitative operations involved in various kinds of conservation . . . : conservation of distance and of length, of area and interior volume, and conservation of congruences in the process of transitive comparisons, etc. The second level might be called the achievement of simple metrical operations . . . : the measurement of length of one, two or three dimensions, the construction of metric coordinate systems, and a first beginning in the measurement of angles and areas. The final level is reached when areas and volumes are calculated . . ., [this] being the level of formal operations. Only now do we find mathematical multiplication being used to coordinate the results of logical multiplication and simple measurement and only at [the last stage] is there conservation of volume relative to the surrounding spatial medium (p. 389).

Yvonne Brackbill

MOORE, THOMAS VERNER (Carthusian). *Heroic Sanctity and Insanity: An Introduction to the Spiritual Life and Mental Hygiene*. Grune and Stratton, New York, 1959. x + 243 pp. $5.00

"If one who is a stranger to Christian thought reads these pages," states the author, "he will find it difficult to realize the fullness of their impact." This book, by a Roman Catholic priest who is also a distinguished psychiatrist, addresses itself primarily to the Catholic reader. The first half of the book is a self-help manual in mental hygiene which draws exclusively on Roman Catholic sources. In brief, Father Moore propounds the thesis that everyone should "shape his or her life to the making of a saint." That is, each must strive to pass "through the temptations of life without ever having committed a mortal or even a fully deliberate venial sin." *Heroic sanctity* is the fulfillment of this ideal. Those who achieve it, contends Father Moore, will be relatively immune to mental breakdown. In the text he explains and illustrates with copious examples the way to heroic sanctity. The non-Catholic reader will encounter unfamiliar terminology and ideas often difficult to follow. However, to all those interested in broadening their transcultural psychiatric horizon, Father Moore's book provides a stimulating introduction to the Roman Catholic point of view. Clearly, a psychiatrist who undertakes the treatment of a Roman Catholic, or any other patient for that matter, must familiarize himself with the cultural background of his patient and the associated sources of emotional security

and self-esteem. It is in this sense that these pages have practical clinical significance for the non-Catholic psychiatrist.

In the second half of the book Father Moore explores the relationship between heroic sanctity and insanity (*sic*); he gives examples of insanity that caricature heroic sanctity, of heroic sanctity that prevails in spite of insanity, and of mental disorders which impair the capacity for sanctity. To the greatest extent he considers all of these issues in the context of a psychobiographical essay on St. Thérèse of Lisieux. Father Robo, another biographer of St. Thérèse, contended that she was the victim of some type of mental illness. Father Moore's discussion is a vigorous polemic against Father Robo's point of view.

Father Moore has a rather dim view of psychoanalysis. It is only fair to warn the nonanalytically-trained reader that most serious students of psychoanalysis would find grave errors in Father Moore's discussion of this subject.

Louis Linn

McCarthy, Raymond G., ed. *Drinking and Intoxication*. (Selected Readings in Social Attitudes and Controls.) The Free Press, Glencoe, Illinois, 1959. xix + 455 pp. $7.50

The purpose of this excellent collection of essays is clearly stated in the introduction: "The custom of social drinking continues, and it must be suspected that even today it serves a need better than anything else so far discovered. It is this practice of social drinking that the present volume seeks to examine, in an effort to understand better its motivation and its results, to define the areas where control is needed to meet the problems associated with drinking, and to point the way toward methods of control which are likely to be successful."

The editor of this volume has been eminently successful in fulfilling his avowed purpose. The subject matter is well organized and remarkably comprehensive in scope. It consists of five parts: Physiological and Psychological Effects of Alcohol; Drinking Practices, Ancient and Modern; Drinking Practices, U. S. A.; Cultural, Religious and Ethical Factors; Controls. Each of these sections is introduced by a general summary of the problem and is concluded with a list of critically selected supplementary readings. The essays themselves have been discriminately selected by a person who is obviously conversant with every aspect of his subject, and the volume is tastefully illustrated.

This book will satisfy a large audience, but will be particularly useful as a source book for physicians and social workers interested in the problem of alcoholism. In the opinion of the reviewer, these essays represent the most complete and authoritative treatment of the social aspects of alcoholism to be found in a single volume.

Maurice Victor

David, Henry P. and Brengelmann, J. C., eds. *Perspectives in Personality Research*. Springer Publishing Company, New York, 1960. ix + 370 pp. $7.50

This second "Perspectives" volume under the auspices of the International Union of Scientific Psychology is an outgrowth of the 15th International Congress in Brussels (1957). Its purpose, to promote international communication among personality researchers, is set forth by Joseph Nuttin (Belgium) in his aptly titled closing chapter, "Beyond Provincialism." In contrast to the earlier volume, *Perspectives in Personality Theory*, the present one focuses more, though by no means entirely, on empirical studies.

No single volume can provide more than the most rudimentary link among the diversely oriented researchers in widespread areas. The present volume is no exception. One does get a taste of divergences in research preferences—say, between the cybernetic influence in the U.S. (Blum) and the physiological influence in the U.S.S.R. (Luria), or between the carefully objective

personality studies of Brengelmann (England) and the highly speculative discussion of religion and psychopathology by Mailloux (Canada) and Ancona (Italy). Perhaps more striking than the divergences are the similarities; the issues of behavior description and depth inference, of holism and analysis, of cross-sectional and longitudinal assessment, and of personality change are represented in the work from all countries. The volume is uneven in its overall quality and frequently loose in its organization; one wonders about the basis for selection of particular papers from the International Congress for inclusion in this volume.

What is particularly interesting, apart from the content of specific papers, is the impression one gets of national trends in research strategy and style of thinking. In his introduction, "Historical Trends in Personality Research," Henry Murray (U.S.) puts himself in general agreement with Gordon Allport (who introduced the first volume) on the point that Anglo-American psychology is generally more positivistic, more methodologically and statistically oriented, than is the more holistic and phenomenological research on the European continent. These national differences can be seen in the present volume without too much difficulty. Yet, by and large, the research area itself seems to co-determine the research style. For example, the Americans in this volume who write on "person perception" tend to be somewhat more descriptive and less method-oriented than those writing on psychodynamics (Miller on denial and Alexander and Adlerstein on attitudes towards death). This seems to reflect a reaction to a research tradition as well as to a national tradition. The person perception researchers explicitly draw from research on object perception, but react against being bound by its more mechanistic tradition; by contrast, the researchers in psychopathology seem to be reacting against the looseness of the psychoanalytic-observational tradition.

The book is divided into three major sections. The first, entitled "Explorations in Behavior," is a loosely put together collection of articles on development and psychodynamics. Sylvan Tomkins (U.S.), in his commentary on this section, does a masterful job of pulling out the linking threads among the articles to describe what he sees as the major trends in the study of psychopathology: the study of social context, of affective processes, and of mechanisms for processing information. Luria's article on "Experimental Analysis of the Development of Voluntary Action in Children" should be read particularly by research extremists of one or another school for its comfortable merging of physiological and Pavlovian concepts with a detailed observational approach reminiscent of Piaget.

In the second major section, on "Person Perception," the articles form a more unitary whole—making this section certainly the fullest resource in the volume. Each of the four papers here (by From of Denmark, Taguiri of the U.S., Taft of Australia, and Hollander of the U.S.), as well as the summary by Robert MacLeod (U.S.), has many areas of overlap. Person perception studies as they emerge here can stimulate a wide range of research—from traditional studies of perception and judgment on the one hand to studies of transference in the psychoanalytic setting—a special case of person perception with reduced cues—on the other. Hollander's article, in which he examines social conformity in part in terms of accuracy of person perception, well illustrates the way in which this area of research crosses other problems.

The final major section is on "Resources," *i.e.*, methods of research. Except for a paper by Shneidman and Farberow (U.S.) on suicide, the papers here are reviews illustrating the longitudinal approach (Skard of Norway and others), objective studies of personality (Brengelmann of England), and projective tests (David and Rabinowitz of the U.S.). In his overview of the issues in personality assessment, Richard Meili (Swit-